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CABLE-MAKING FOR SUSPENSION BRIDGES AS EXEMPLIFIED IN THE EAST RIVER BRIDGE.

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III.

III. THE MATERIAL AND THE MANNER OF WORKING IT INTO THE CABLE.

1. *The Cable Wire.*—In all bridge cables, constructed previous to those of the East River bridge, charcoal iron wire of either No. 10 or No. 9 gauge was used. The cables of the Niagara railway bridge, for instance, contain 3640 wires No. 10 gauge with an ultimate strength of 2658 tons, forming cables of 10 inches diameter. Those of the Cincinnati bridge—until now the largest suspension bridge ever built—are 12 inches in diameter and consist of 5200 No. 9 wires with an aggregate strength of 4212 tons. In comparing with these the cables of the East River bridge, we find that the strength required for the latter exceeds over two and a half times the strength of the former cables. If composed of the same material, their bulk and the time required for making them would be increased in the same proportion, two items, which it is desirable to have reduced as much as possible. It was therefore concluded to manufacture these cables of steel-wire. In order to determine the size of the wire and the most advantageous quality of steel, very thorough experiments, continued over a series of years, were made with all kinds and grades of steel-wire of different sizes in regard to their tensile strength and ductility. As the result of these tests a wire was decided upon, as described in the following extract from the specification:

"The wire must be made of steel of superior quality, must be hardened and tempered and, lastly, must be galvanized.

The size of the wire shall be No. 8 full Birmingham guage. A length of 14 feet must weigh exactly one pound before it is galvanized.

"Each must have a breaking strength of no less than 3400 lbs. This corresponds in wire weighing 14 feet to the pound, to a rate of 160000 pounds per square inch of solid section. The elastic limit must be no less than $\frac{41}{100}$ of the breaking strength, or 1600 pounds. Within this limit of elasticity it must stretch at a uniform rate, corresponding to a modulus of elasticity of not less than 27,000,000, nor exceeding 29,000,000 pounds.

"All wire must be 'straight' wire;

that is to say, when a ring is unrolled upon the floor, the wire must lie perfectly straight and neutral, without tendency to spring back in the coiled form, as is usually the case. This straight condition must not be produced by the use of straightening machines, but by a patented process, which consists in leading the wire, from a point within the galvanizing trough, in a straight line under considerable tension to the guide sheave or winding drum, and in locating the drum at such a distance as to permit the wire to be cooled and set before it is coiled thereon.

"There will be two kinds of tests:

"*First.* A piece 60 feet long, from every fortieth ring, is placed in a vertical testing machine which has a vernier guage, capable of being read to $\frac{1}{10000}$ of one foot, so attached as to indicate the stretch of 50 feet of the wire. An initial strain of 400 pounds is now applied, which by successive increments of 400 pounds is increased to 1600 pounds. The amount of stretch for each of these increments shall be the same, and the total stretch between the initial and terminal strain shall not be less than $\frac{97}{1000}$ of one foot equal to $\frac{194}{100000}$ of the 50 feet. And furthermore, on reducing the strain to 1200 pounds, there shall be a permanent elongation not exceeding $\frac{1}{100000}$ of its length.

"The minimum strength of the wire, if subjected to a breaking strain, shall be not less than 3400 pounds, and the minimum stretch, when broken, shall have been two per cent. in fifty feet, and the diameter of the wire at the point of fracture shall not exceed $\frac{15}{100}$ of one inch.

"A piece 16 inches long, cut from every ring and a piece 6 feet long from one ring out of five, will be subjected to the same tests and must come up to the same standard.

"*Second.* Every ring will be subjected to a bending test, by cutting off from each ring a piece of wire one foot long and coiling it closely and continuously around a rod one-half inch in diameter, when, if it breaks, it will be rejected."

The contract, based on the above conditions, was awarded to J. Lloyd Haigh, of South Brooklyn, for $8\frac{7}{10}$ cents per pound, with the distinct understanding that none but crucible cast steel shall be used for the wire.*

The wire is delivered in rings of 60–70 pounds, containing 800–1000 feet of the smaller, and 600–800 feet of the larger size.

Before being used, each ring receives three coats of oil, by dipping it first in a tank with raw, and then twice in one with boiled but cold linseed oil. Each coat of oil must be perfectly dry, before a new one is put on. After the oil has thoroughly hardened, the rings are ready to be spliced.

The splice used formerly for iron wire consisted in tapering the ends of the wires for about $2\frac{1}{2}$–3 inches, laying the flat sides upon each other and wrapping the whole with thin wire. By a blow on a specially prepared steel mould, the circumference of the wire received a number of small nicks or notches, which prevented the wrapping wire from slipping. This splice proved fully equal to the strength of No. 9 iron wire; but applied to heavy steel wire it averages, if well made, only a strength of about 24–2700 pounds, hence is insufficient for wire exceeding 3400 pounds. Experiments made with different kinds of splicing gave the preference to a screw-coupling, which by the smallest bulk had the greatest percentage of strength, and finally the splice, as illustrated in Fig. 29, was adopted for the East River bridge cables.

* The wire furnished by the contractor exceeded considerably the required ultimate strength, being in average 172,000 pounds per square inch. After two strands were completed with this wire, it was concluded to increase the size of the wire to 11 feet to the pound, equal to No. 7 Birmingham guage. This wire possesses a little smaller breaking strength, averaging about 170,000 pounds per square inch, but greater ductility. The principal advantage however, is, that instead of 332 wires, only 282 are necessary for one strand, which gives a saving in the time of manufacture of nearly one-sixth.

Fig. 29

The screw threads at the end of the wires have this peculiarity, that the thread instead of being cut in the rod, is raised above it, so that between each turn there is a short straight piece of rod. This result is obtained by tapering the ends of the wires a little and turning a cylindrical screw on this cone. By a screw length of $\frac{1}{2}$ inch for each wire, this coupling gives about 95 per cent. of the strength of the wire. After the ends of the wires are secured in the ferrule, the coupling is cleansed with a solution of potash, then dipped in molten zinc mixed with a little tin and finally painted with red lead.*

2. *Strandmaking.*—After a number of wire rings are spliced together, the wire is wound on the wire drum, passing on its way through a piece of sheepskin saturated with oil. Every thing else pertaining to traveling rope, shoe and leg connection, etc. being prepared, we are ready to stretch the wires for the strand.

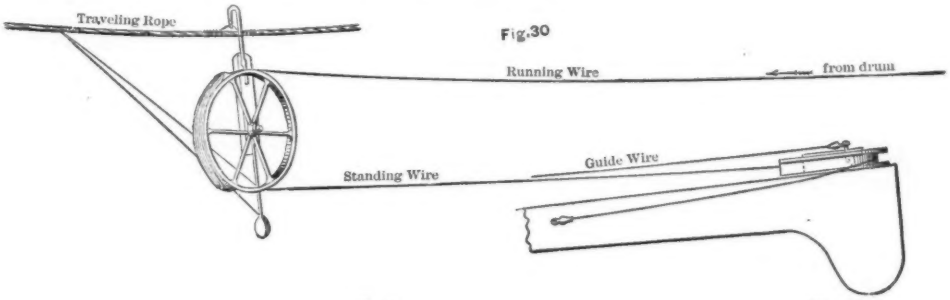


Fig.30

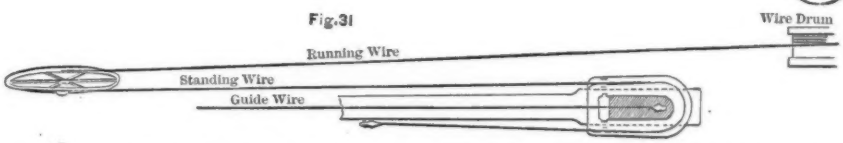


Fig.31

Figs. 30 and 31 show the rear part of the leg with the shoe and guide wire in position.

The end of the wire, with which the strand is started, is fastened to the side of the leg, passes around the shoe over the traveling sheave and back again to the drum. The lower wire, leading from shoe to traveling sheave, is called "standing wire," the upper, which uncoils from the drum and travels with twice the speed of the working rope, "running wire." In order to avoid confusion, it is necessary to preserve this distinction throughout the strand, and to keep each set always on the same side of the shoe. The standing wire occupies the inside, next to the traveling shoe, the running wire the outside of the sheave. The sag in the wires, while traveling across (see Fig. 1) is regulated by a brake on the wiredrum. As soon as the traveling sheave has passed the first tower, the standing wire is placed in the saddle and regulated in the first landspan. This is done by simply pulling it over the tower until it hangs parallel to the guidewire, and holding it in its place by a temporary twine lashing to some convenient object on or near the saddle. The running wire in the meantime travels on, supported by small wooden rollers outside of the saddle. After the traveling sheave has passed the second tower, the standing wire in middle span is immediately regulated in the foregoing manner, so that after its arrival at the second anchorage, only the last land span remains to be adjusted. This being done, the running wire is next thrown on the opposite side of the strand, and then regulated in the same way as the standing wire, but working in opposite direc- Brooklyn anchorage is the starting point; tions. At the East River bridge the hence all standing wires are regulated from Brooklyn towards New York, and the running wires from New York to-

* To avoid the unscrewing of the wire from the ferrule during its passage across the river, which sometimes happened, the ferrule receives a little notch at each end, which being filled with zinc adhering to the wire, prevents its turning.

wards Brooklyn. For adjusting the wires a pair of nippers as illustrated in Figs. 32 and 33 are used, and the hauling is done with a small block and fall fastened to some convenient place on the anchor-chain. While the regulation of these two wires is going on, the traveling sheave, which carried them over, returns empty and another sheave fastened to the opposite side of the working rope, brings two wires for the strand of the second cable.

Fig.32

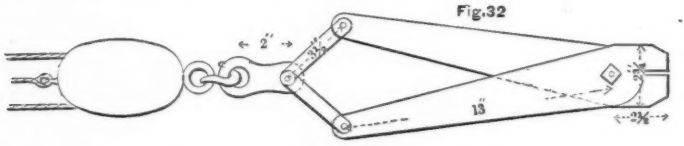
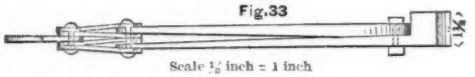


Fig.33



Scale ½ inch = 1 inch

About nine or ten days are required to lay a strand of 280–300 wires, if no delays happen. After the last wire of the strand is in place, it is cut and spliced to the end of the first wire so that the whole strand is formed of one continuous wire. The latter operation is a delicate one, because the ends have to be cut so that the deflection and length come exactly right. A few trials with a temporary splice are therefore necessary to be made.

The next operation consists in tying the two parts of the strand together so as to form a round, solid little cable, which at the East River bridge has a diameter of 3½ inches. For this purpose a so called "carriage," as illustrated farther below in Figs. 43 and 44, is placed on the strand on top of the towers, in which a few men slowly descend towards the anchorages and center of river, tying during the passage the strand every 16–24 inches with four turns of No. 14 annealed galvanized wire. Before being tied, the wires are sqeezed together by means of a pair of tongs, on the claws of which an iron sleeve, by knocking it towards the end of the arms, exercises great squeezing power.

The strand is now ready to be "let off." This consists in relieving the shoe from its temporary seat on the leg, and to let it forward in its final position, at the end of the anchor-chain. It is an operation, which, with heavy strands, requires great care and foresight, because an accident here would not only be a loss of many thousands of dollars, but also would cause immense damage to property and shipping, and likely great destruction of life. It is therefore advisable, not only to make careful calculations, but also to have every rope and iron used in this operation thoroughly tested.

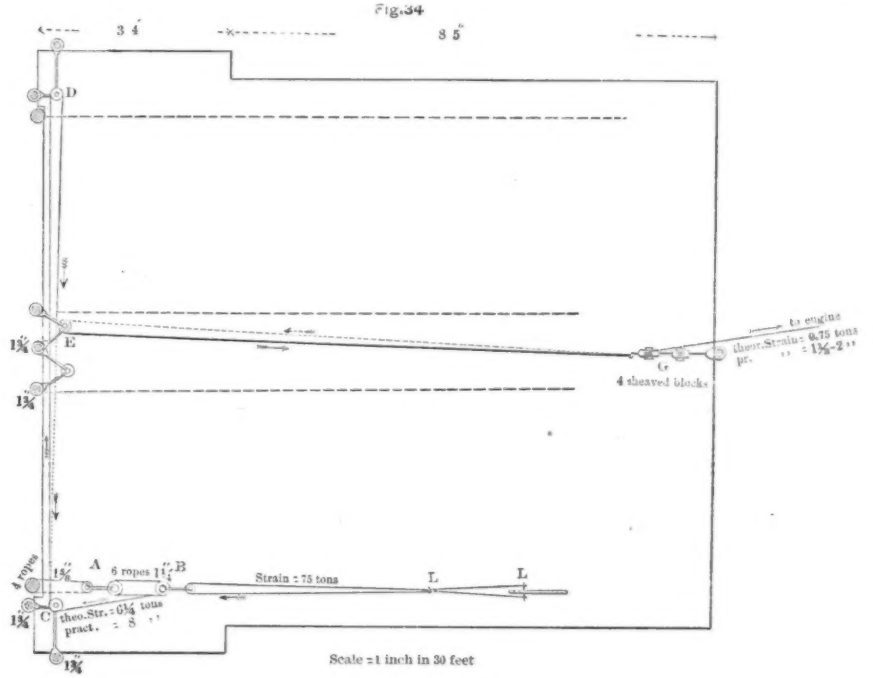
Fig. 34 represents the top view of the anchorage, with the full letting off arrangement for one of the outer strands of the East River Bridge.

Two heavy iron bars *LL*, fitting closely to the sides of the shoe, and bolted to a double steel plate in front of it, form the grasping arrangement. Figs. 35 and 36 show this on a larger scale.

At the end of the iron bars, are four 8 inch sheaves, which are attached by means of eight 1⅝ inch iron wire ropes to the four-grooved small sheave of a pair of blocks *B* and *A*. One of these is illustrated in Figs. 37 and 38.

Block *A* is the standing block, fastened stationary to a heavy log in the rear of the anchorage, by means of four 1⅝ in. wire ropes. It contains six iron sheaves of 23 in. diameter, connected to the sheaves of the running block *B* by means of a twelve folded 1¼ in. steel rope, the fall of which after passing around several rollers *C*, *D* and *E*, is attached to a four sheaved wooden block *G* at the other end of the anchorage. This wooden block is worked by a 1½ in. manila rope, which connects with the drum of a steam engine. This arrangement of course, may be changed to suit different local requirements. In the present case, its advantages are: first, that the wooden block *G* moves only in the middle line of the anchorage for letting off any strand of the four cables, this middle part being the only one free from obstructions; second, the length of the

Fig.34



Scale = 1 inch in 30 feet

Fig.35

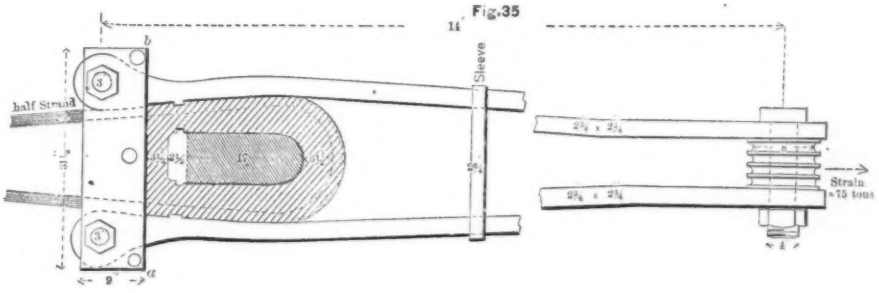
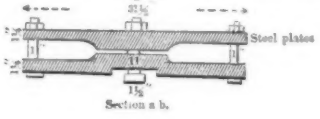


Fig.36 Scale ½ inch = 1 foot



Section a b.

anchorage not being sufficient for moving the shoe in one process the entire distance, it is necessary to do it in two processes, viz., to arrest the motion as soon as the wooden block arrives at the rear end, to take the fall rope from the intermediate sheave *E*, and to place it from *B* directly over *C*, occupying in Fig. 34 the position of the dotted line. With this the double distance *C E*, is gained in the length of the fall rope, and the wooden block can be placed again so much back, to continue the operation. An arrest of the motion and a relieve of strain in the fall rope, can easily be procured, by lashing the twelve ropes, between standing

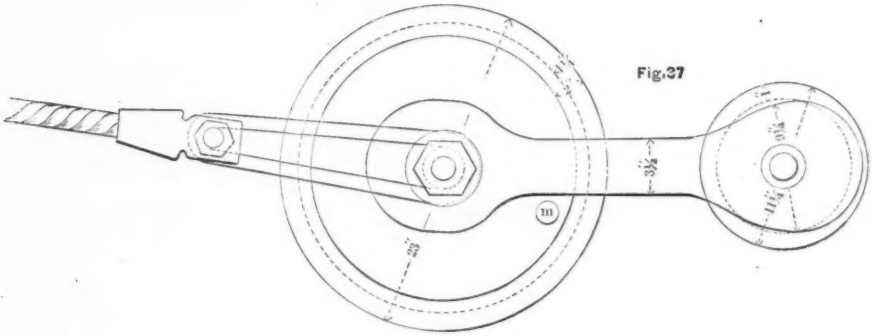


Fig. 37

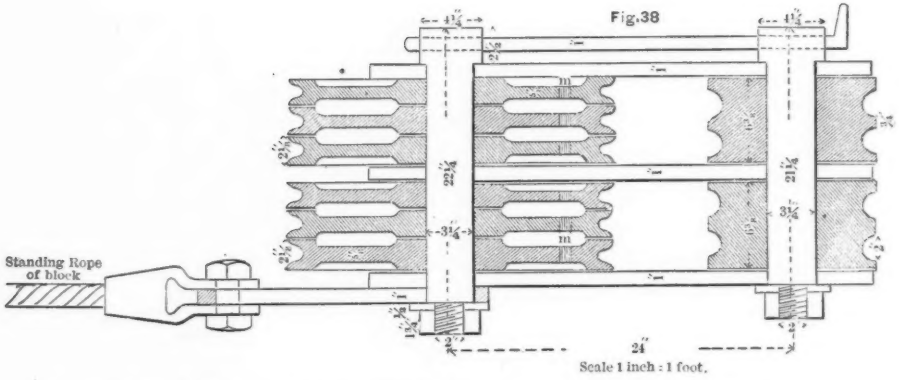


Fig. 38

Scale 1 inch : 1 foot.

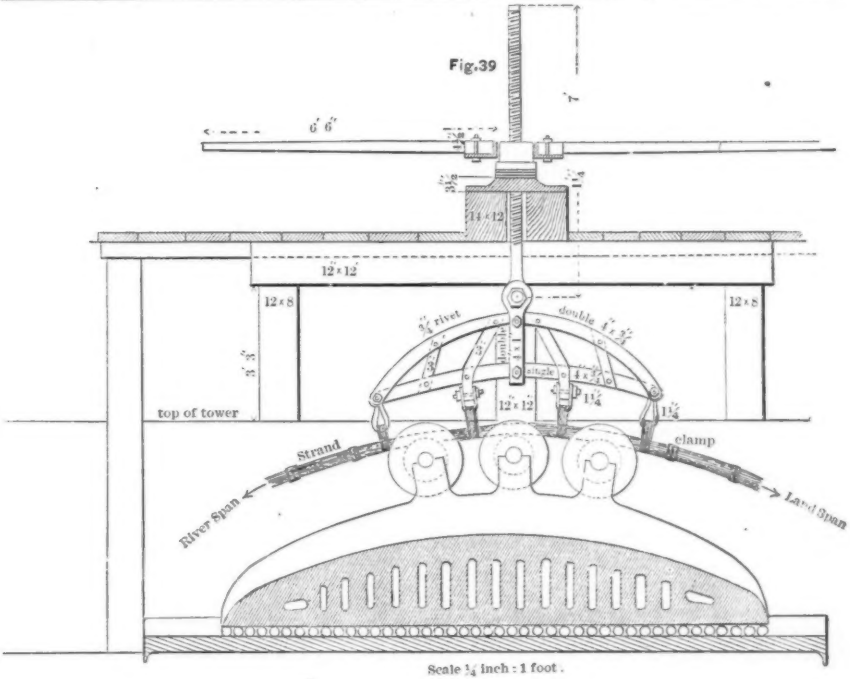
and running block, tightly together. The tension in the strand is 75 tons, that of the fall rope, consequently $\frac{75}{12} = 6\frac{1}{4}$ tons + friction, equal together to about 8 tons, and that of the manilla rope $\frac{6\frac{1}{4}}{8} = \frac{3}{4}$ tons + friction, or about $1\frac{1}{2}$ to 2 tons. The shoe must move 12 feet, hence the wooden block $12 \times 12 = 144$ feet, and the manila rope $144 \times 8 = 1152$ feet.

The operation commences in pulling the shoe $\frac{1}{8}$ of an inch back from its seat on the leg, and to raise it above the latter in order to get it free. As the strand itself pulls upward, this rise of the shoe requires no extra force. The arrows in the diagram (Fig. 34) show the direction in which the different ropes move during the first operation. As soon as the shoe is free, and the motion of the engine reversed, it slowly travels forward until it reaches the eyes of the anchor links, through which a short pin is driven for the shoe to rest against. After having the shoe secured in this way, the tension in all the ropes is slackened, but the whole letting off apparatus is left intact, because it is needed again for regulating the strand.

Lowering the Strand in the Saddle.—This operation is illustrated in Figs. 39 and 40.

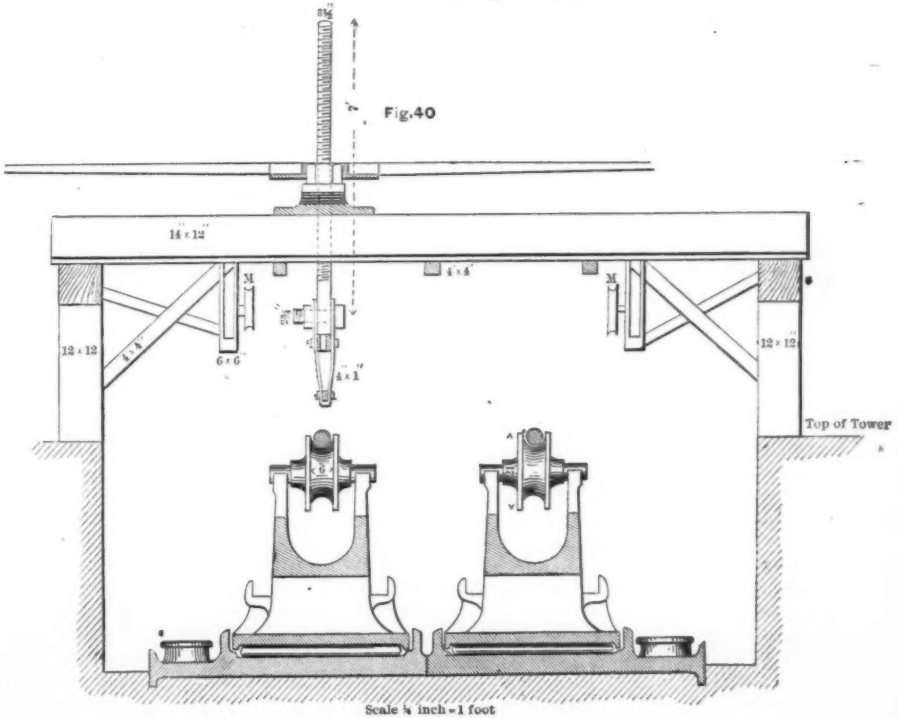
Two 14×12 inch timbers, supported on posts, resting on top of the masonry, bridge over the pit in which the saddles are placed. They give support to the nut of a $3\frac{1}{2}$ inch screw of hammered iron, to the lower end of which a yoke-shaped iron frame is attached, which takes hold of the strand at different points, extending over a length of about six feet. The nut, shown on a larger scale in Fig. 41, is turned by means of a cast iron spider (Fig. 42), which fits over the nut and which is provided with six open troughs for the insertion of wooden levers. As soon as the strand is lifted clear over the flange of the rollers, the latter, which rest in open bearings, are

Fig. 39



Scale ¼ inch : 1 foot.

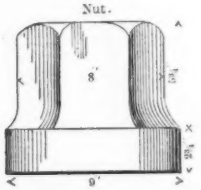
Fig. 40



Scale ¼ inch = 1 foot

taken away and the strand is slowly lowered in the saddle.

Fig. 41



This operation finishes the work on the single strand, which is ready now to be regulated. The first strand of each cable theoretically needs no regulation, being parallel to the guidewire throughout. But practically there will always be some slight differences in their relative position, owing to inevitable small inaccuracies. It is, therefore, necessary to select the shortest as the guidestrand, and to adjust the others accordingly. The shortening is done in pulling the shoe back from the pin, and inserting between the two an iron segment of the necessary thickness.

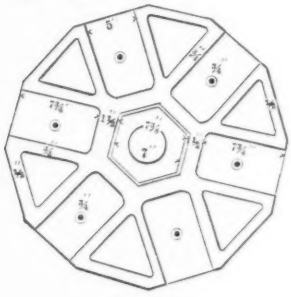


Fig. 42

The strands of one cable, after being regulated, must occupy a relative position as shown in Fig. 5. In the middle span, where all strands hang parallel, there is no particular difficulty in doing this, but in the landspans, where the strands diverge from tower to anchorage, some special considerations are necessary for a successful execution. The leading idea must be, to adjust each single strand so that they all come just right if squeezed together and combined into a solid cable. As the land cable must form a balancing curve to the river cable, it is necessary that each strand shall do the same. Calculating, therefore, to each strand of the river span the equivalent curve of the landstrand, which passes through a certain fixed point, (the point where the cable leaves the anchorage), we are able to compute the length of the strand between this point and the point of intersection. To this length must be added the distance from the face of the anchorage, to the point of attachment to the chain. These two together will give the total length of the landstrand, which will balance the corresponding river-strand. Instead of measuring actually this length, to do which accurately would be impossible, we establish again tangent lines for the regulation, in the manner described for the guidewire. It is therefore necessary to find an equation between the coördinates of the curve and consequently the following problem must be solved: A parabola of a given length passes through to two fixed points, which have the horizontal distance B and vertical distance h; the position of the vertex shall be determined. Calling the coördinates of this imaginary vertex in regard to the upper fixed point y and x, we have the following two equations:

$$y\left\{1+\tfrac{2}{3}\left(\frac{x}{y}\right)^2\right\}+(B-y)\left\{1+\tfrac{2}{3}\left(\frac{x-h}{B-y}\right)^2\right\}=S=$$

known length of curve, (1)

$$\frac{y^2}{x}=\frac{(B-y)^2}{x-h} \qquad (2)$$

(equation of the parabola).

In (1) are the higher powers of the small fractions $\frac{x}{y}$ and $\frac{x-h}{B-y}$ neglected.

It follows from it:

$$\frac{x^2}{y}+\frac{(x-h)^2}{B-y}=(S-B)\tfrac{3}{2}=m$$

or $x^2B-2xyh+h^2y=mBy-my^2$

from (2) follows:

$$x=\frac{h\,y^2}{2\,By-B^2}$$

The value of x substituted in the foregoing equation, we obtain:

$$\frac{h^2y^4B}{(2By-B^2)^2}-\frac{2\,h^2y^3}{2\,By-B^2}+h^2y=mBy-my^2$$

or:

$$y^3+y^2\left\{\frac{6h^2B^2-8mB^3}{-3h^2B+4mB^2}\right\}+y\left\{\frac{-4B^3h^2+5mB^4}{-3h^2B+4mB^2}\right\}+\left\{\frac{h^2B^4-mB^5}{-3h^2B+4mB^2}\right\}=0.$$

The expressions in the brackets are constant, and we may call them a, b and c, so that the equation will obtain the form:

$$y^3+ay^2+by+c=0.$$

This is a simple equation of the third degree, which can be solved by introducing a new unknown quantity z, which is determined through the equation:

$$y=z-\frac{a}{3}$$

Calling for abbreviations:

$$b-\frac{a^2}{3}=u \text{ and } \frac{2}{27}a^3-\frac{b}{3}a+c=v$$

and substituting all these values in the above equations it receives the following shape:

$$z^3+uz+v=0.$$

From this follows as one of the roots:

$$z=\sqrt[3]{-\frac{v}{2}+\sqrt{\frac{u^3}{27}+\frac{v^2}{4}}}+\sqrt[3]{-\frac{v}{2}-\sqrt{\frac{u^3}{27}+\frac{v^2}{4}}}$$

Calling the two parts of this expression k and n, the three roots of the equation are:

$$z_1=k+n$$

$$z_2=-\frac{k+n}{2}+\frac{k-n}{2}\sqrt{-3}$$

$$z_3=-\frac{k+n}{2}-\frac{k-n}{2}\sqrt{-3}$$

If two of these roots should be found irrational, of course the rational one is the correct value of z. But if all three are rational, only that value is correct which satisfies equations (1) and (2). Having determined z it is easy to find x and y or the equation of the curve, by means of which the tangent lines can be fixed. The actual calculation is very laborious as it contains large multiplications and figures in the 4th and 5th, power, but, on the other hand, the successful regulation of the strands is at stake, without which united action in the cable would not exist, or at least would be very uncertain.

3. *Wrapping the Cables.*—As soon as the last strand is in position, the cable will be wrapped. The whole wrapping apparatus consists of three parts: the wrapping carriage (Figs. 43 and 44);

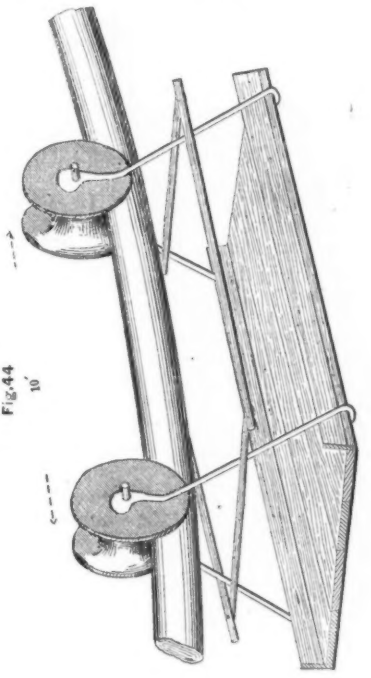


Fig. 44

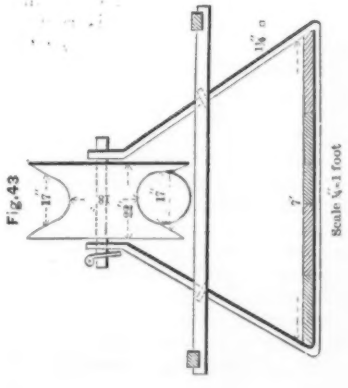
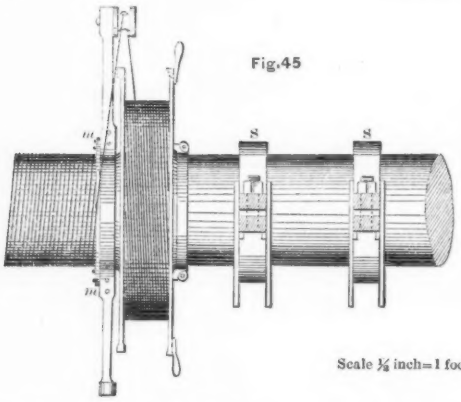
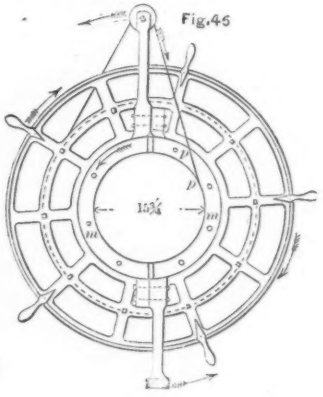


Fig. 43

the squeezers (*ss* in Fig. 45) and the wrapping machine proper (Figs. 45 and 46).

A "carriage" is first placed on each cable at both sides of the towers. It is similar in construction to the one used





for tying the strands, but having rollers wide enough to run on the cable. The object of this carriage is to serve as working platform for the men who handle the wrapping machine.

The squeezer consists of two half round steel-bands $1\frac{7}{8}$ inch thick by $3\frac{1}{2}$ inches wide, which fit on the cable or rather which, when placed on the loose strands, force the wires of the cable to assume a circular form. The upper part of the squeezer is provided with four ears, screwed to its side, which diverge somewhat towards their lower end in order to facilitate its slipping on and to catch easily all the wires. The squeezing is done by means of two 2 inch screw bolts which pass through the flanges of the band. Using long wrenches it is possible to exercise great power. Before applying the squeezers, the temporary wrapping of the strands is removed, at least from all within reach. At the East River bridge cables this squeezing and wrapping will be done in two processes. First the seven inside strands will be treated like a separate cable, squeezed and wrapped, before the other strands are manufactured. Afterwards the operation will be repeated with larger squeezers and wrapping machines. In this manner the temporary wrappings of all the strands, save the middle one, can be cut and removed, which would not be possible if the loose strands of the whole cable had to be wrapped. Besides, this double process has the advantage of allowing tighter squeezing with less power, and altogether makes surer work. The cutting of the strand lashings is done a few feet in advance of the two squeezers, which are placed about 10 or 12 inches apart, the first one as near as possible to the saddle. Before tightening up the screws of the squeezers, the cable is once more saturated with oil, so that all the little spaces between the wires are completely filled and hermetically closed. The wrapping machine is now placed a short distance behind the first squeezer, and when the wrapping has reached the latter, this squeezer is put 10 inches in advance of the second one, which in turn is placed the same distance ahead as soon as the wrapping has proceeded so far, etc.

The wrapping machine (Figs. 45 and 46) consists of a drum, formed of two light cast iron frames connected by wooden shrouding, which revolves on a cast iron barrel, that under considerable pressure can slide on the cable, but otherwise is immovable. On the same barrel, but independent from the drum, a ring $\overline{mn}$ with a steel facing revolves, which has two arms, one having at the end a little roller, the other a weight. In the steel face of this ring, there is a small groove $\overline{pp}$, which runs in the line of the tangent from the circumference of the cable to the just mentioned roller. The whole apparatus is in two halves, which are screwed together after being

placed on the cable. The wrapping wire, which is coiled on the drum, passes from here over the roller, through the groove, to the cable where its end is fastened. Now the drum is turned in direction of the arrows and the two armed ring is turned in opposite direction. The first motion uncoils the wrapping wire, while the second winds it on the cable and at the same time keeps it by the momentum of the counter weight under great tension. As the wire passes through the groove in the steel ring, it squeezes itself, on account of the spiral lay, between the finished wrapping and the barrel and, consequently, pushes the latter ahead for a distance equal to the thickness of the wrapping wire. The ends of these wires are spliced, so that the whole wrapping in one span consists of one continuous wire. When the two wrapping machines meet in the center of the span, they are taken off and the two wires joined. Care must be taken, to wrap in opposite direction, so that the ends of the two last wires are on opposite sides of the cable and can be spliced under tension.

The wrapping wire generally consists of No. 11 or No. 10 annealed iron wire. For the East River bridge No. 10 galvanized wire will be used. Near the saddle, where the wrapping machine cannot be placed, the cable is bound together with iron bands about five inches apart. At the point in the anchorage, whence the different strands diverge, a heavy iron ring in addition will be placed, in order to lessen the strain on the wrapping wire. The single strands are wrapped by hand from this ring close to the shoe. Immediately after having wrapped the cable for a certain distance, it should be painted with several coats of good oil paint, so that the little spaces between the wires are completely filled up, giving to the cable a smooth cylindrical appearance and preventing the moisture to penetrate.

This operation finishes the cables, which are now ready to receive the superstructure. This should be put on symmetrically to the towers, and simultaneously in centre and land spans, in order to avoid uneven tension in the cable, and strain on the towers.

After the greater part is suspended, the blocking, which held the saddles in place, is removed, giving to the cable free chance to move back or forth, and to assume its natural position of balance.

We have shown that under a certain temperature, this motion will be about two inches towards the river. The temperature under which the first strand of the East River bridge cable was regulated, happened to be 75—80° F. If the saddle blocking is removed on a colder day, the above motion will be less owing to the greater tension in the land cable, caused by greater contractions, than the river cable. The reverse will take place if the saddles are set free on a warmer day. Condition, however, always is that in each span, the weight of the superstructure is the same, and symmetrical to the tower.

The lengths of the suspenders must be calculated according to the final curve of the cable, which is attained when the stays are put in place. Before this the floor will have an irregular shape, provided all suspenders have their exact length. A continuous grade of the floor will, however, be established as soon as the stays are put under tension, which will cause the cable to assume that curve for which the suspenders were calculated, bringing thereby, the latter also under their proper tension.

At a meeting of the Engineering Society, King's College, London, held lately, Mr. C. D. Marr read a paper "On Marine Engines." The author in commencing expressed regret at being unable, on account of the short time at his disposal, to say anything about the early history of the marine engine and the respective merits of the various kinds of propellers, and other points closely connected with the subject. He first described the various kinds of paddle engine, dwelling particularly on the oscillating engine; and spoke also of injection and condensation. He then passed on to the screw engine, and described the most recent examples of inverted and horizontal return connecting rod engines, noting especially the vertical engines of the screw steamship San Francisco, belonging to the Pacific Mail Steamship Co. of the United States of America, treating of the surface condensers. He then drew a comparison between the injection and surface condensers.

TRANSPORTATION OF HIGH EXPLOSIVES.

Written for VAN NOSTRAND'S MAGAZINE.

THIS subject has now come to be one of acknowledged importance; and, owing to peculiar circumstances, its serious consideration is pressingly demanded.

On the one hand, we find these explosives are being manufactured and used on a large scale, and their transportation on public conveyances openly and freely and generally is very desirable; while, on the other hand, the public and common carriers dread the consequences.

Nitroglycerin in the liquid state is justly deemed too dangerous for anything like general transportation; and its compounds, the high explosive powders, are supposed to be almost equally dangerous.

The laws both of the United States and of the several states on this subject are in a crude, uncertain and inappropriate condition. The result is that proper transportation is greatly restricted and hampered, and improper transportation practiced.

We therefore propose to examine the entire subject with some care.

In doing this we will first consider the nature and character of the explosives themselves, and next, the present state of legislation in regard to them, and finally the general features of a proper law.

But first of all a few words as to the industrial importance of these powders, and the strong necessity for their transport by common carriers. This is already pretty generally understood, but a few facts will not be inappropriate.

These powders vary in strength—the strongest being estimated to be ten times stronger than common blasting powder for disruptive purposes. Hence their great value in hard blasting.

Their use has resulted in what may be properly called a revolution in the art of blasting. The cost of hard rock work is reduced by them fully one-third, and much time is saved besides. With them many undertakings become successful which, without them, either have been, or would be, failures.

No important hard rock work is now done without them. The great silver mines of Nevada and many other mines are worked exclusively by them.

They are also the leading explosives in many of the mines of California, Utah, Colorado, Arizona, Missouri, Michigan, Pennsylvania, New Jersey and Northern New York; and are extensively employed in quarries, railroad cuts and tunnels, canals, aqueducts, river and harbor obstructions, and for the removal of stumps and boulders on farms. Their consumption at the present time in this country is not far from five million of pounds per annum.

Their transport long distances and over a wide range of territory is an imperative necessity. Their manufacture to any considerable extent on the ground where used is impracticable. The materials for their production are found at the great centres of trade and manufacture, and weigh four times as much as the product, while the chief consumption takes place at great distances from these centres.

To prohibit their transportation by common carriers of freight, or to so encumber or restrict their transport by these means as to make it as expensive as to manufacture on the ground, would have the effect to greatly diminish their consumption and so far check the development of that class of industries which deserve special encouragement.

NITROGLYCERIN.

Nitroglycerin, the basis of what are generally known as the high explosive powders, is a light colored, oily liquid, about fifty per cent. heavier than water. It is not soluble in water, and, therefore, when poured into a bore-hole filled with water it sinks to the bottom displacing its bulk of water.

The ordinary mode of making it is—first to mix two and one half parts, by weight, of strong nitric acid with five parts of strong sulphuric acid. After allowing this mixture to cool, there is gradually stirred into it one part of pure glycerin, care being taken to keep the temperature below 70° Fah.

The nitroglycerin is then separated from the mixture and carefully washed in cold water.

This is a mere outline. There are

many important details, not appropriate here.

We shall discuss only one of the qualities of nitroglycerin—its explosiveness.

This we do, because it is absolutely necessary to an intelligent explanation of the structure and nature of the powders, and the difference between it and them.

ITS COMBUSTION.

The first fact to be noticed in this connection is that it does not explode by fire like gun-powder, gun-cotton and the fulminates.

With fire it behaves very much like whale oil. At ordinary temperatures it does not burn by itself. It must have something in the nature of a wick to aid combustion.

Touch it with a match, live coal or a red hot iron, and it burns so long as the contact is maintained, but when these things are withdrawn the combustion ceases.

It must be separated—brought out of its compact form before it will burn.

If mixed with almost any dry substance, whether the substance be itself combustible or not, it will burn.

Mixed with ashes, sand or brick dust, it burns almost as rapidly as when mixed with charcoal, sawdust or even gunpowder, if the gun-powder is saturated with it.

The gases formed by its combustion are materially different from those formed by its explosion.

They are much more offensive to the smell and injurious to health. It sometimes occurs that the exploder fails to explode a high explosive powder, but sets it on fire.

When this takes place in a mine not well ventilated the smoke is almost intolerable, while the gases of complete explosion are far from being so.

More will be said as to the combustion of the powder hereafter. As to the combustion of liquid nitroglycerin, it is enough to say that it is a totally different thing from its explosion.

Its burning is so slow that no more violence comes from it than from the burning of whale oil.

ITS EXPLOSION.

It is a powerful explosive, more so than any other disrupting agent in use. It has, in a high degree, the great requisite for disruptive force, to wit, the ability to furnish abundance of gas rapidly.

However, the superiority does not lie so much in the abundance of gas as in the rapidity of its formation which is done not by fire but by force suddenly applied to all parts of the charge.

This will be fully explained further on.

It is estimated to be from ten to twelve times more powerful than gunpowder. But this mode of expression is loose and liable to mislead. One pound of nitroglycerin will not do what ten pounds of gunpowder can, and ten pounds of gunpowder will not do what one pound of nitroglycerin can. It cannot be used in ordnance or fire arms, as even small charges burst the gun.

In all kinds of hard rock work; also in sub-aqueous blasting; in breaking iron or steel, and in all positions where disruptive rather than ballistic effect is required, nitroglycerin is wonderfully efficient.

But how is it exploded? The ordinary means of exploding it at will, in use, is a heavy percussion cap or exploder.

This consists of a copper shell, like a common gun cap containing ten grains or more of fulminate of mercury. The exploder is not fired by percussion, but by a fuse or electric wires inserted in it, in contact with the fulminate.

It is also readily explodable by mechanical percussion, concussion, jar, shock, blow or vibration.

This is why it is so extremely dangerous.

Its handling in the liquid form at ordinary temperatures has to be performed with extreme caution. Perhaps no better illustration of its dangerous character can be given than by quoting the Rules laid down by Geo. M. Mowbray Esq., and which his customers are warned to obey in its use.

"INSTRUCTIONS FOR HANDLING AND USING MOWBRAY'S TRI-NITRO-GLYCLRIN.

"1. Handle carefully, avoiding a sudden jar or concussion, and be very careful, if any is spilt outside the can, to avoid striking it against any hard substance.

"2. When solid, thaw out by placing the can in a tub of warm water, not hot-

ter than the wrist can bear, first pouring warm water into the can, and always remove the can before adding more hot water to the tub.

"3. To fill cartridges, &c.: hold the cartridges to be filled over a tray, say two feet by three feet, the bottom of which should be covered with plaster of Paris (which will not readily explode when saturated with nitroglycerin). The soiled plaster of Paris should be frequently renewed.

"4. If the nitroglycerin in a liquid state is kept in store or magazine for some time, the cork should be loosely inserted, and a pint of cold water poured into each can, to be frequently poured off and replaced with fresh cold water in warm weather, taking care to retain the bladder under the cork. It is preferable when ice can be procured to congeal the nitroglycerin.

"5. Use funnels (gutta percha if they can be had) for filling water holes. Under no circumstances whatever attempt to tamp the drill holes; it is unneccessary, and may kill the man who attempts it.

"6. Hot irons to warm the water, or for soldering the cans, will be sure to cause explosions.

"7. Never sledge or attempt drilling in a hole or seam where nitroglycerin has been spilled; fire an exploder, which will effectually clear it up.

"8. Never pour nitroglycerin into a hole unless perfectly sure that it is a sound hole, or will hold water; if seamy always use cartridges.

"9. To obtain the best results with niroglycerin, drill deep holes, six feet or more. Use powerful exploders and well insulated wires. It is cheaper to fire by electric battery with simultaneous explosion than to fire several holes with tape fuse.

"10. Look out after a blast for any unexploded cartridges lying around.

"11. Never allow any but the most careful persons to handle or have charge of the nitroglycerin, and insist upon the use of every precaution to prevent an accident or explosion.

"12. Never allow empty glycerin cans to be used for any other purpose, but destroy them by a fuse and exploder, or by building a fire under them, first, however, removing them to a safe distance.

"13. Examine your cans from time to time, and notice if, at the level of the nitroglycerin, any pin holes have eaten through; in such case procure a new can, or stone jar, and empty the contents out, not trusting your hold to the upper part of the can lest it may give way.

"14. When solid, or congealed it is absolutely safe; if possible, therefore, any surplus should be stored surrounded with ice, since no explosion can take place when it is solid."

To this may be added a few cases of accidental explosion. At Yonkers, a boy threw a stone at a can of it, and was killed by the explosion.

At Aspinwall, a can of it fell the distance between decks and exploded, doing immense injury. In opening a package of it by Wells, Fargo & Co., in their office in San Francisco, it exploded, killing many persons and wrecking the building. At Worcester, Mass., a quantity of Dualin in a car exploded doing great mischief. By some it is thought that this explosion was caused by exploders, which were in the same car with the powder, while others suppose it was due to a leakage of nitroglycerin from the powder amongst the springs and axles of the car and upon the rails.

While being transported in a wagon from Titusville, Pa., it exploded, destroying horse and driver. While opening a tin cartridge of it last summer at Hell Gate, it exploded, and killed three men.

In Greenwich Street, New York, a can of it exploded from voluntary decomposition.

While being transferred last year in a pail from the tank to the scales and mixing pan, it exploded, killing two men and destroying the factory.

There is also more or less danger from its instability. When it contains acids it is liable to change. A chemical action sets in which may result in combustion and, under confinement, in explosion.

The rapidity of the change depends upon the quantity of acids present and the temperature.

The greater the proportion of acids and the higher the temperature the more certain and rapid the action.

A failure to thoroughly cleanse the oil from the acids used in its manufacture may lead to its voluntary decomposition.

This was doubtless the cause of the

disastrous explosion in Greenwich Street. The can was observed to be smoking, and was taken from the house and placed on the sidewalk, where it soon exploded.

Many other like proofs might be added, but these are enough for our purpose, to wit, to show that liquid nitroglycerin cannot be handled and transported with any reasonable degree of safety. *They justify our conclusion that nitroglycerin ought not to be stored, transported, or used in the liquid state.*

Nobel made known his method of exploding nitroglycerin by an exploder in 1863, but the transportation and use of the liquid was not really commenced until 1865, and was discontinued almost completely in 1867. In less than two years' time, in spite of every precaution, it had destroyed so many lives and so much property as to alarm the whole civilized world. Both in this country and abroad stringent laws were enacted, designed—some of them to regulate, and others to suppress, its transport and use. None of these laws were too stringent; many of them not enough so. The plain truth is that while the liquid form is retained no amount or kind of packing can render it secure against the shocks, jars, vibration and breakages incident to transportation.

THE DYNAMITE INVENTION.

This consists broadly in mixing nitroglycerin with any dry, pulverized, solid substance, in such proportions that on the one hand the mixture will be an effective explosive powder, and on the other hand will not leak or part with any portion of its nitroglycerin. This is known by the generic name of Dynamite. In this country it is called by different names by different manufacturers.

Giant Powder, Rend Rock, Hercules Powder, Vulcan Powder, Jupiter Powder, Neptune Powder, Thunderbolt Powder, Dualin Powder, Titan Powder, Titanite Powder, Potentia Powder, Vigorite Powder, are some of the names it bears.

All these powders, known as the high explosive powders, are essentially the same thing.

Nitroglycerin is in them all and is the leading element. The difference between them consists in the different proportions of nitroglycerin which they contain, and the different solid materials used, and the different proportions of these solids.

These solid materials when prepared for admixture are called the "absorbent," "dope," or "dust," and are always in the dry and pulverized state.

Let us first consider the general features of these powders, and afterwards some of the circumstances by which they are affected; and first of all let us take up the question of safety, as this is the all-important one now before us.

THEIR SAFETY.

We might content ourselves with the external proofs on this subject, such as actual tests by experts and experience in transportation and use, but there is a natural craving to know how a thing is constituted—a laudable desire to look into the structure and see whether there can be found there any internal and intrinsic evidence in support of the external testimony.

Besides, as all these powders contain nitroglycerin, and it is admitted that nitroglycerin is extremely dangerous, the mind irresistibly carries the danger with the nitroglycerin into the powders, and especially is this the case when all agree that the powders are explosive, and solely so by reason of the nitroglycerin they contain.

How is it that the nitroglycerin retains its explosive power and not its sensitiveness—its danger? If it will explode in a bore-hole, why not out of a bore-hole?

If it will explode for use, why not for mischief? How can it be sensitive when you wish it to be so, and not so when you do not wish it?

Here is a seeming paradox.

These are pertinent questions, and ought to be answered, not by mere assertion but by logical deductions.

To be clear to the unprofessional reader, we must proceed by degrees slowly.

Let us suppose we mix water with gun-powder. What is the result? The gun-powder is still there in the mixture, but is it dangerous? It is wholly inexplosive, not even combustible.

This answers the general assumption that what is dangerous alone, is so in mixture. Again, let gun-powder be mixed with fine sand or ashes or pulver-

ized glass. What is the result? This mixture is incombustible also.

This was the celebrated Gale invention.

It was proposed to transport gunpowder in this mixture and carry it in this form upon the battle-field and on ships of war until required for use, and then to sift out the fine, inert material and leave the gun-powder clear.

But old war dogs preferred the extra danger to the uncertainty and delay of the separation, and the scheme died.

But it shows what mixture will do.

Now let us suppose we mix an *ounce* of nitroglycerin into a *pound* of pulverized infusorial earth, charcoal, chalk, ashes, or any other absorbent. What is the result?

Like the gunpowder mixtures it is wholly inexplosive and wholly incombustible.

Here is nitroglycerin but where is the danger? No means on earth can explode such a mixture.

Now let us increase little by little the proportions of nitroglycerin, and keep trying to effect an explosion; and for this purpose, make use of the most certain means, to wit, the exploder already described.

We shall at last obtain a mixture which is explodable by means of this exploder. The mixture will be comparatively dry. It will be a powder. It will not be a liquid or pasty, nor wet or damp; it will not leak or lose its nitroglycerin, however long it may stand. It will also be a powerful explosive. Now try to explode it by blows upon it. Hit it in mass with hammer and sledge. Let it fall from great heights. Drop heavy weights upon it from great heights. Set it on fire and burn it. Abuse it; turn it; throw it; heat it; freeze it; wet it; dry it; do almost anything you please with it, except to put the strong exploder to it, and you cannot explode it.

This is dynamite—or a high explosive powder.

Now let us go a little into details. The first inquiry is, Will a powder made on this plan be a really powerful explosive?

One would naturally suppose, that if it contained no more nitroglycerin than is sufficient to make it explosive at all it would be very weak and ineffective.

Here appears one of the wonderful peculiarities of nitroglycerin.

In mixing inert pulverized substances with any of the old fire explosives its explosive power is diminished *in proportion to the adulteration*—the direct effect being to slow the combustion. The greater the proportion of the adulterant the slower the combustion until the mixture becomes not only inexplosive but even incombustible, as in the Gale invention already noticed.

On the other hand, the absorbents used in nitroglycerin powder do not strictly slow its explosion at all.

This difference is due to the different laws under which the two classes of explosives explode.

The gunpowder class or fire explosives decompose by combustion progressively—the charge burning from the point of ignition step by step until all parts are reached and consumed.

It is apparent that in this kind of explosion its speed depends entirely upon the facilities for the passage of flame, and anything which obstructs its passage slows the explosion. The fine sand or pulverized glass used by Gale had this effect.

It filled up the interstices between the grains of powder and separated them from each other, and thus prevented the spread of the flame and heat, and rendered the mass wholly inexplosive when the proportion of inert matter was sufficient and slowed—retarded the action in the exact proportion to the amount of dust present.

On the other hand, nitroglycerin does not thus explode by the application of fire, but by the application of force. Whether we call it shock, jar, a blow, percussion, concussion or vibration or the heat produced by these, one thing is plain, that force is necessary in some form, whereas in gunpowder explosion it is not.

This force is applied not gradually or slowly or to a part of the charge at a time, but at once, and suddenly, and to all parts of the charge simultaneously. Perhaps it may be said that this cannot be strictly, theoretically true where a small exploder is used in a large charge—that the exploder cannot affect directly the entire charge but explodes a part of it and this explosion explodes the bal-

ance. For the purposes of the present inquiry this is immaterial. All that is essential to be sure of in this connection is, that the explosion is produced by force whether applied directly by the exploder or indirectly by propagation.

We will therefore proceed.

In order to the explosion of nitroglycerin at all, whether in the liquid or powder form, a certain degree of this force is requisite; and when this proper degree of force is applied to its particles they explode however situated.

The fact that these particles are separated one from the other is of no consequence so long as they are made to feel the requisite force.

For example; If a thin film of the oil be placed upon the face of an anvil, or on sheet metal, and be struck so as to compress the particles of the oil they explode. Now if we have a series of these plates one above the other, to any extent with the oil between them all, and a blow be struck upon the top of the pile all the nitroglycerin between the plates throughout the pile will be exploded, substantially simultaneously, and yet the charge thus exploded was completely divided into as many wholly separate parts as there were plates.

Whereas if gunpowder had been placed between the plates instead of nitroglycerin and fire had been applied at the top of the pile, the explosion would have been limited to the burning of the powder between two plates only.

Suppose now, we use plates of india-rubber, or wood, or cloth, or leather, or any other compressible material, instead of the rigid plates of iron. What is the effect?

A blow upon the top of the pile does not produce the percussive, sharp, ringing and violent effect requisite to explosion. The blow is deadened, the nitroglycerin is cushioned and protected and no explosion follows as in case of the iron plates.

Here is the whole secret of the dynamite invention. When an absorbent is used it acts like these soft plates in separating, cushioning and protecting the nitroglycerin, and making it safe against mechanical blows; but when sufficient force is applied to solidify the absorbent, as when an exploder is used, it acts like the iron plates in communicating the force to the different parts of the charge.

The absorbent renders the oil less sensitive, that is to say, more force is required to explode the nitroglycerin in the powders than to explode it in the fluid form.

While in the fluid form it is compact and incompressible, and slight force will give it the requisite compression; but when in the powder form it is cushioned by the absorbent, and this cushion has to be compressed and made solid, and this requires additional force precisely as it requires a more forcible blow to drive a nail whose head is cushioned. The cushion must be compressed to solidity before the nail will start.

However, notwithstanding the explosion is more difficult when the nitroglycerin is thus cushioned, yet, when the force is sufficient to effect it at all, the decomposition takes place from the same cause and proceeds in the same way and is accomplished in substantially the same time as when not so cushioned; and therefore the intrinsic power developed by the nitroglycerin in the powder is substantially the same as when the same amount of it is exploded in the fluid form.

Thus we find that from the peculiar nature of nitroglycerin a powerful powder can be made, and at the same time be extremely difficult to explode, and therefore safe.

For instance: A powder made fifty per cent. of nitroglycerin and fifty per cent. of infusorial earth is very dry and cannot be exploded except by a triple force exploder, and when the charge is strongly and tightly confined.

On the other hand, a powder made fifty per cent. of nitroglycerin and fifty per cent. of mica scales or fine sand, is very wet and leaky and explodes almost as easily as the liquid oil. Nevertheless the earth powder is in every way as strong as either of the others. This confirms our position, that a powder which is very dry and difficult to explode, and therefore safe, is substantially as good as a wet and therefore dangerous one, when the proportion of nitroglycerin is the same in each.

Having now shown the principles upon which these powders are constructed, and having demonstrated thereby that

they may be so made as to be safe for transportation, handling and use, let us consider the chief circumstances by which they are affected.

CHARACTER OF THE ABSORBENT AND PROPORTION OF NITROGLYCERIN.

From what has been already said, it is plain that these powders may be so made as to be either safe or unsafe according to the proportion of oil they contain. But it must be borne in mind that this proportion is to be measured not arbitrarily by percentage merely, or weight, but relatively according also to the absorbent capacity of the solid substances used.

Of course, different substances have different absorbent capacities. Some will hold safely seventy-eight per cent. of nitroglycerin, while others will not safely hold two per cent.

Wetness or dryness, then, is the true test of safety. If it leaks it is dangerous; if not it is not dangerous.

Here it may be asked whether a powder, which is as damp with nitroglycerin as it can be without leaking, is not dangerous?

We answer, no. A moment's thought will settle this, as it has been settled by experience. All mixtures so fully saturated as to have no vacancies will leak.

The fact that it does not leak shows that it has pores, interstices, vacancies; that it is not compact like the liquid, but compressible and yielding. This shows it to be safe. Its safety does not depend so much upon the *amount* of vacancies in the powder as upon the fact of their existence at all. A slight compressibility destroys the fatal rigidity and gives safety.

Infusorial earth with eighty per cent. is saturated, and almost as compact as pure nitroglycerin and almost as dangerous, while with seventy-five per cent. it is abundantly safe for the highest torrid zone temperature. Even a powder which will leak will be safe against blows upon it.

Its danger lies in the escape of the liquid.

The oil drained from powder is as dangerous as if it had never been in it.

Here, then, we find the exact spot upon which the law should lay its hand.

No leaky powder should be transported at all. It should be classed with liquid nitroglycerin. To determine whether a powder is dangerous is the simplest of all things. If it will leak it is dangerous, if not, it is safe.

Whether it is leaky or not is settled by an examination.

It would be folly to rely on the proportion by actual weight of the ingredients used, because, with the same proportions, but with absorbents of different capacity, one powder would be dry and safe and another wet and dangerous.

Actual inspection and test of the powder is the only reliable criterion.

Here it should be noted that too much importance is liable to be attached to this point of leaky powders. The actual danger from this source, in the absence of all legal regulations, is very slight. No manufacturer or dealer in these powders would, knowingly, send abroad a leaky article—it would be condemned on sight. This matter is now thoroughly understood, and the practice of making the powders dry and safe is settled beyond danger of disturbance; and the likelihood of their being made and sent abroad in a leaky condition by accident is very remote.

Self interest is a security that may generally be relied upon with confidence.

Only one leaky powder has ever been heard of, and that doubtful. We refer to the Worcester case. However, this state of facts is no excuse for neglecting to pass laws against leaky powders.

TEMPERATURE.

Other things being equal both nitroglycerin and its compounds explode easier as the temperature is higher.

At any temperature below zero Fah. liquid nitroglycerin is not explodable by any ordinary means. At 32° Fah. it explodes with great difficulty. This is the means employed mainly by Mr. Mowbray for making his nitroglycerin safe for handling and transport. He freezes it. However, frozen nitroglycerin *can* be exploded. All that is necessary is to intensify the means. Use a sufficiently strong exploder or confinement or both, and it can be exploded at any temperature.

So much for the effect of cold. The other extreme is about 360° Fah. At this temperature it either burns or ex-

plodes. If free from all pressure, jar, vibration or force in any form, it burns, otherwise it explodes.

Placed in a film on tin, and held over a spirit lamp, it smokes away or takes fire and is consumed. But heated to this temperature when of any considerable depth, say over a quarter of an inch, it explodes.

When heated to any degree less than this, it is exploded by a cap, or blow, or jar, or vibration, with an ease proportionate to the temperature. At 350° the fall upon it of a dime will explode it.

Temperature has another special influence on the powder, to wit, they are more liable to leak as their temperature is raised.

A powder which would be dry and safe at 50° may be leaky at 100°.

The powders, therefore, should be made with reference to the highest temperature to which they are to be exposed, and when tested they should be at this temperature.

CONFINEMENT AND COMPRESSION.

Other things being equal, nitroglycerin and its compounds are more easily exploded, as they are more closely confined and strongly compressed. If they be enclosed without pressure, and an exploder applied, the enclosure aids the exploder in applying the pressure, so that a charge which a certain exploder will not explode in the open air may be exploded, by this same exploder, in a bore, hole or under water.

So also, if they be not only tightly but also strongly inclosed, as in gas pipes, with a cap screwed on each end and set on fire by a fuse through a small hole or otherwise, they will explode. The gases from the combustion cause a pressure on the part of the charge not burned which, unless they escape, will finally be sufficient to cause explosion.

Compression of the charge makes it more sensitive.

Let an iron tube be filled with nitroglycerin.

A blow, which will not cause explosion when struck against the upper end of this tube, will cause explosion when struck against its lower end, the difference being that the lower part of the charge is under pressure from the upper part. But this pressure from superincumbency has no material influence on the powder for reasons too obvious to be mentioned.

On the other hand, if spread upon an anvil and struck, only so much as is hit, explodes; the balance does not, because it is not sufficiently confined or held so as to receive the requisite blow or pressure from that part which does explode. The reason why a small quantity of nitroglycerin in a large mass of absorbent cannot be exploded at all, is, that the absorbent cushions the liquid so deeply that the requisite pressure is not felt.

The practical lesson from these facts is that the powder for transportation ought not to be packed in strong and tight vessels.

METALLIC CASES.

The rigid character and peculiarly forcible vibration of steel and iron, and especially when in the sheet form, seem to be particularly favorable to the explosion of nitroglycerin and its compounds.

What will not explode a charge in wood or paper will explode it in iron.

Place a three inch charge of dynamite in the bottom of a hole bored into a log, and fire three inches of gunpowder well tamped above it and the dynamite will not explode; but in iron, it will.

Strike nitroglycerin in a leather bag and it will not explode by a blow which will explode it in a tin vessel.

Nobel deemed it of great importance that the solid form of dynamite allowed it to be packed in wood; this because the metallic vessels in which the liquid was forced to be carried were so dangerous.

Mr. Mowbray gives the following instance; A part of a can of nitroglycerin was placed 300 or 400 feet away from a blast in the Hoosac Tunnel. The upper edge of the can was in contact with the iron rail leading from the vicinity of the blast. The blast exploded the nitroglycerin in the can. This was undoubtedly from the vibration along the rail. If the oil had been in wood or paper, it would not have exploded.

This teaches us not to use metallic cases for the transportation of nitroglycerin or its compounds.

THAWING AND INCLOSURE.

So far as known there are but two

ways in which there is any danger from fire.

One is where the powder is completely inclosed in some strong vessel and set on fire, as already explained. The vessel must be much stronger than any in which the powders are ever transported, and must be so tight that the gases can not escape as fast as they form.

There is no practical danger from this source in transportation.

The other way is in roasting, toasting and baking the powder, when frozen.

When frozen cartridges are put into a hot oven, upon stoves or boilers, or in kettles over a fire, or placed before a hot fire and allowed to remain long enough to thaw, and become so hot as to smoke, the result is that in about nineteen cases out of twenty the powders take fire and burn up, or all the nitroglycerin is evaporated, and they are ruined.

But in the twentieth case there will be an explosion. If all the powder is equally exposed to the heat, as the evaporation commences long before the exploding point is reached, the powder is weakened, and the explosion is correspondingly weak—often a mere pop or puff.

But if there is a large quantity of powder, some of which is thus heated, and the balance left unaffected, the explosion may extend to the full strength powder, and be violent accordingly.

But as this mode of heating frozen powder cannot take place during transportation, it does not belong in the discussion, and is only mentioned for the sake of having the review complete.

SPONTANEOUS DECOMPOSITION.

Liquid nitroglycerin, as already stated, unless completely freed from the acids employed in its manufacture, is liable to voluntary decomposition, especially in warm times and places, resulting in most cases in combustion, but under favorable circumstances, in explosion.

By the oversight, or carelessness of a workman, this danger may be incurred. But this instability is not found in the powder. No instance of such decomposition has ever been known.

Besides, it has been shown by repeated experiments that nitroglycerin impregnated with acids, and which will decompose while in the liquid form, may be made into powder, and subjected to all the conditions most favorable for decomposition, without that result.

The powder form, that is, the solid form, seems to be an effectual bar to the peculiar chemical action requisite for spontaneous decomposition.

EXPERIMENTAL TESTS.

Having considered the structure of the high explosive powders and the circumstances which affect them, let us briefly review some of the actual experiments touching their safety.

We believe there has never been any question but these powders are abundantly safe from explosion by fire. The following are common tests which may be repeated by any one at any time.

If set on fire in piles, large or small, either loose or in cartridges, it burns up rapidly like chaff when loose, but slowly like rosin, tar or sulphur, when in cartridges.

Set fire to one end of a cartridge and it burns much like a Roman candle, without the pop but with less speed.

When partly burned it may be extinguished by water or the foot.

As packed for transportation in boxes of inch boards strongly nailed and set on fire by a fuse through a gimlet hole, its gases spring the boards apart and the flame issues. A box of one-hundred pounds is burned in from two to five minutes, according to the composition of the powder. Those who know the powder stand upon the box while it is burning.

Poured upon a red hot iron it burns.

A red hot iron thrust into it sets it on fire.

The Steamer "Meteor" took fire from its furnace and burned to the water's edge on Lake Erie consuming 8,000 pounds of Giant Powder on board without explosion.

The only source from which the enemies of these powders profess to apprehend danger during transportation, is percussion, concussion, shocks and blows.

The following experiments have been repeated many times:

A minute quantity is placed on an anvil and struck with a hammer. A snapping sound is produced like the breaking of a stick, and the particles be-

tween the points in contact are exploded and no more—the balance is scattered.

If the quantity be increased, and the blows repeated until the powder is made solid, a greater quantity can be exploded; but if the quantity is so large that the blows are deadened and the percussive action prevented there will be no explosion.

A cartridge of powder laid upon a rail and passed over by cars will be cut in two, and the small amount between the wheel and rail will be exploded, if not by the first wheel, by subsequent ones, but the balance of the cartridge will not explode.

Pounding it upon wood will not explode it.

A box of it thrown from any height upon rocks will be broken in pieces, but no explosion can be thus caused, nor by dropping heavy weights upon it. A box of it between car buffers, meeting at a speed of sixty miles an hour, will not be exploded.

If special preparations are made it can be exploded in quantity by percussion.

By putting it into an iron cylinder, fitted with a piston, it can be exploded by blows on the piston sufficiently numerous and heavy. But, of course, this cannot occur in transportation.

A car loaded with iron rails at full speed collided with a car of powder, and the rails were driven through the boxes and powder without exploding it.

ACTUAL EXPERIENCE.

The manufacture of dynamite commenced in 1867 and the trade in it has gradually increased until it now amounts in this country and Europe to about 15,000,000 pounds per annum.

Its transportation has taken place by all the ordinary means, on vessels and vehicles of every description. It has met with such kinds of treatment as would naturally befall an article supposed to be safe in every respect and handled roughly. Nevertheless, there has never been an accidental explosion from any cause while being transported either in this country or abroad, except the Dualin at Worcester already mentioned.

There have occurred several explosions supposed, at least to some extent, to have been due to these powders but the supposition is unwarranted.

To avoid all misunderstanding we will enumerate them. The most notable case was the Bremerhaven explosion. This was not of dynamite proper but of a compound resembling it, called Lithofracteur. However, the powder was not to blame, as the explosion was caused by an exploder placed in the powder by design, and fired by clock work.

The Bergen Hill explosion was of what is known as Rend Rock powder, and was caused by exploders applied by workmen during a strike.

An explosion of dynamite took place in San Francisco while being prepared for a large blast under water. The loose powder which was being packed into cartridges was set on fire from the pipe of a workman, and before it could be extinguished the fire reached exploders in other cartridges near by and of course, caused their explosion.

The explosion last year at Drakesville was of liquid nitroglycerin, while being weighed and transferred.

The fact that it occurred at a dynamite factory led to the report that it was dynamite.

AUTHORITY.

As would be naturally expected with a substance so novel, powerful and useful as these powders, they have received careful attention and study, and been subjected to a variety of tests in all quarters with reference to their safety.

Men of science; experts in explosives; men in charge of works requiring their use; committees on behalf of transportation companies; military gentlemen and government commissions, and numerous others have examined the subject, and reported upon it,—some of them elaborately.

It would be inappropriate to reproduce these reports here or even extracts or their substance in detail. They are before the public and can be referred to by those who desire to do so.

We must content ourselves with a brief summary of the conclusions from all of them, to wit:

1. These powders are the most powerful of all the disrupting agents now in general use.

2. When properly made and economically transported they are, for hard rock work and sub-aqueous work, by far the most economical explosives in use.

3. They are the safest of all explosives both in transportation and use, many times safer than gunpowder, and when properly made and a few simple precautions taken, are as practically safe for transportation as if they were wholly inexplosive.

4. There is no good reason why, under proper regulations, they should not be transported in freight conveyances as freely as any ordinary merchandise.

STATE OF PRESENT LEGISLATION.

The congressional enactments now in force on this subject were passed July 3, 1866, before dynamite was invented, and are found in secs. 4,278, 4,279, 4,280, 5,353, 5,354 and 5,355 of the Revised Statutes.

Their design was to regulate the transportation of nitroglycerin, which is a liquid, but by accident they were so drawn as apparently to embrace all compounds containing nitroglycerin. Nevertheless, in a case before the U. S. District Court for the Northern District of New York, it was settled that dynamite or Giant Powder, was not within the law. But there is no certainty that a like result would be reached in all cases.

This uncertainty is itself a serious objection, and ought to be removed.

If the law applies to the powders they cannot be conveyed on public conveyances unless packed in metallic cases, and these cases be surrounded by plaster of Paris and their outside be conspicuously marked, "Nitroglycerin—Dangerous."

From what has gone before it is plain that each one of these requirements is the exact reverse of what it ought to be.

As to the metallic cases, they were necessary for liquid nitroglycerin but are not so for the the powders; besides, as already shown, they are more dangerous than wood or paper.

As to the plaster of Paris, its purpose was to absorb the nitroglycerin in case of leakage from the metallic vessel. But in making these powders this absorption has already taken place fully, and to guard against leakage from a dry thing is simple folly.

It will be noticed that this law recognizes the dynamite principle as the basis of safety.

As to marking the powders "*Nitroglycerin—Dangerous*," it would be a double falsehood.

They are not nitroglycerin, nor are they dangerous, any more than wet gunpowder is gunpowder and dangerous.

This death's head and cross bones sign, marked conspicuously on a box of the powders, or on a dry goods box would prevent its reaching its destination as effectually as if marked "*Death to him that touches.*"

The practical result of these absurd and useless requirements has been their own *felo de se.* Not a pound of these powders has ever been carried in metallic cases, or surrounded with plaster of Paris, or marked "*Nitroglycerin—dangerous.*"

They are carried either by private conveyance at immense extra expense, or on public freight conveyances without being packed or marked as the law requires, or smuggled on passenger conveyances and by express under false names.

This shows that the law, so far as it affects these powders is worse than useless. But, on the other hand, if it does not apply to these powders there is no law of Congress that does, and they are free to be carried at pleasure on passenger conveyances, and in the midst of dense populations dripping with nitroglycerin—a state of things too alarming to require further comment.

This subject has already been considered by the Legislatures of several States, some of which have passed laws upon it substantially like the congressional one, and others have not, mainly because of their limited application. State laws are limited in their operation to such cases of transport as begin, continue and end within the State, while almost all transportations of these powders extend beyond a single State, and are, therefore, exclusively within congressional jurisdiction.

The great vice of the laws now in force is that they recognize no difference between liquid nitroglycerin and these powders.

Under the law as it now stands, the liquid can go where the powder can, and the powder cannot go where the liquid cannot.

Such a law as this ought not to stand another day.

The practical world has, for ten years

past, thoroughly recognized a difference —why should not law makers do so?

If the claims now made against nitroglycerin are true, ought it longer to be permitted to be transported in the dangerous manner laid down in the law?

And if the claims made in behalf of the powders are true, ought they to be classed with liquid nitroglycerin?

And if the law does not apply to the powders, and they are free to be carried on passenger conveyances, even when in a leaky and dangerous condition, ought not a new law against this to be enacted forthwith?

There never was a case where the necessity for immediate and intelligent action was more apparent and pressing than in this.

If, what we have written is true, our conclusions are undoubtedly just and proper; if, what we have written is not true, it can be easily disproved; but until it is so disproved the duty of at least a careful examination of the subject is palpably clear.

GENERAL FEATURES OF A PROPER LAW.

We now come to the practical question as to what should be the nature of the regulations on this subject.

They are few, simple and obvious:

First: As liquid nitroglycerin is known to be dangerous—so much so that no prudent manufacturer asks or attempts to transport it—and especially as there is now no necessity for it—its transportation on public conveyances and by common carriers should be absolutely prohibited, not only on passenger conveyances but also on freight conveyances.

Second: Leaky powders ought to be classed with liquid nitroglycerin, and wholly debarred from transportation in like manner.

Third: The dry and safe powders ought to be carried on all public freight conveyances.

The test of safety should be this, that when at the highest temperature to which they are to be exposed during their transit, they will not leak or part with any portion of the nitroglycerin which they contain when so placed that it can drain therefrom if it will.

Fourth: These powders, even when dry and safe, ought not to be transported on passenger conveyances. The ground on which this regulation ought to be made is that the powders are *explosive* and the persons of passengers ought not to be subjected to *any* risk however remote from this source.

However safe the powders there would be *fear*. To the timid and ignorant this fear would amount to intense pain. Passengers ought not to be thus tormented.

Besides there is no necessity for transport on passenger conveyances if one by freight is open.

Fifth: The powders ought not to be packed in metallic cases. The fact that metallic cases add almost nothing to the danger and that no accident has ever occurred from this cause, is no good ground against the regulation.

Sixth: Large percussion caps or exploders, or other things whose explosion by fire will explode the powders ought not to be transported in conjunction with them. If the Worcester explosion was caused by the exploders on board it is a case illustrating the propriety of this regulation.

Seventh: Each separate package ought to be marked on the outside with the name of the contents so as to be legible to those who handle it.

These are all the regulations which an experience of ten years has suggested as necessary.

Some of these are almost useless but none of them absurd like most of those now in force. They are suggested out of abundant caution and because of the extremely sensitive state of the public mind on the subject of nitroglycerin.

Under these regulations these powders will be a hundred fold less dangerous than gunpowder and we may confidently hope and expect there will be no accidents whatever in their transport.

DRAINING OF THE ZUYDER ZEE.—A dam 40 kilometers (24¾ miles) long, 50 meters broad at its base and 1½ meters above the usual level of high water, is to be carried across the gulf. Upon this will be erected pumping engines capable of discharging 1,716,000,000 gallons a day. Estimating the average depth of the gulf at about 4½ meters, it will make steady pumping for 16 years to empty the enclosure.

THE RATE OF SET OF METALS SUBJECTED TO STRAIN FOR CONSIDERABLE PERIODS OF TIME.

By Prof. ROBERT H. THURSTON.

Transactions of American Society of Civil Engineers.

Section I.—On the Observed Decrease of Resistance at a Fixed Distortion.—The writer has, in a preceding paper shown, by reference to experimental researches in which he had then engaged, that some classes of metals, as ordinary iron and steel, when subjected to strain and distortion by a force exceeding the resistance of the material within the elastic limit, take a set and are stiffened by that act, and exhibit an exaltation of the elastic limit. It was also shown that other classes, like tin, and similarly viscous and ductile materials, exhibit flow and a depression of their limits of elasticity when similarly treated. It was further shown, that the former class when subjected to loads, even approaching their ultimate strength, took a certain set and remained apparently indefinitely without further distortion; while the second class, under very moderate loads, frequently exhibit a gradual yielding, a progressive distortion, until fracture took place, sometimes under stresses which were but a fraction of those which were found required to break such metals quickly, and when time was not allowed for flow to occur. It was noted that increase of rapidity of distortion and fracture produced increase of resistance in the latter, or "tin-class," and decrease of resisting power in the first, or "iron-class," and *vice versa*.

The writer has since instituted experiments upon metals of both classes to determine how rapidly set, in each class, took place; the earlier experiment just referred to, having confirmed a suspicion long existing among engineers and experimentalists, that the phenomenon was a molecular change, as well as of the mass, and that time was required for its complete development. Prof. Norton has also shown by experiment that this set is partially temporary, the bar relieving itself of distortion in some degree, on removal of the load. Both that experimenter and the writer had detected some peculiar variations of form during this recovery, and the experiments of the latter, as detailed in the preceding paper, exhibited at times a gradual recovery of straightening power in a confined and flexed bar. The following will be found interesting, and perhaps, important, as showing how these molecular changes progress.

Bars were prepared of square section, 1 inch in breadth and depth, and 22 inches in length, between bearings. They were flexed in a machine for testing the resistance of materials to transverse stress, as described in the preceding paper and the load and deflection carefully measured. As the bars were retained at a constant deflection, their effort to resume their original form gradually decreased, and the amount of this effort was, from time to time, noted. When this effort or resistance had become considerably decreased, the bar was released, and the set measured. This operation was repeated with each, until the law of decrease of elastic resistance was detected. Curves were constructed, illustrating graphically this aw, and exhibiting in more satisfactorily and more plainly than the tubular record.

The following is the record for the bars of iron, of tin, and of two alloys. The iron bar No. 648, was subjected to a load 1,003 pounds, somewhat less than one-half its maximum, and its deflection was found to be 0.0995 inch. Removing the load, the set was 0.0049 inch. Restoring the load (1,000 pounds, + 3 pounds due to the weight of the bar), the deflection was 0.1001 inch, and the bar was held at this deflection and the decrease of resistance observed. In 25 minutes, it had become 999 pounds; in 1 hour 40 minutes, 991 pounds; in 4 hours 35 minutes, 987 pounds, and in 5 hours 20 minutes, 987 pounds. The set was then found to be 0.007 inch under the weight of the bar itself.

Restoring the last observed load, the deflection was 0.0991 inch, and the origi-

nal load of 1,003 pounds increased it to 0.1003 inch.

A second trial of the same bar under a load of 1,603 pounds gave a deflection of 0.2548 inches, and a set, on removal, of 0.1091 inch. Restoring the load, the deflection became 0.287 inch, and the resistance to flexion decreased in 6 hours 3 minutes, from 1,603 to 1,457 pounds, at which latter time the set was found to be 0.1451 inch. Restoring the load of 1,457 pounds, the deflection was 0.2863 inch, and the original load, 1,603 pounds, being brought upon it, its deflection increased to 0.3016 inch, an increase nearly twenty per cent. above the original deflection.

In the first trial the loss of stiffness, as measured by the decrease of effort to straighten itself, and which is here taken to measure the *rate of set*, is seen to have been nearly proportional to the time at first, becoming constant after 4½ hours. On the second trial, after a considerable set, produced by a heavy load, the set became constant after about one hour, and so remained to the end of the trial.

No. 655 was a bar of Queensland tin, received from the Commissioner of that country at the Centennial Exhibition, and which was found to be remarkably pure. A load of 100 pounds gave a deflection of 0.2109 inch, and produced a set of 0.1753 inch. The same load restored, deflected the bar 0.2415 inch, which deflection being retained, the effort to regain the original shape decreased in one minute from 100 to 70 pounds, in three minutes to 62, and in eight minutes to 56 pounds. The original load of 100 pounds then brought the deflection to 0.3033 inch, nearly fifty per cent. more than at first.

A bar, No. 599, of copper-zinc alloy similarly tested, deflected 0.5209 inch under 1,233 pounds, and took a set of 0.2736 inch after being held at that deflection fifteen minutes, the effort falling, meantime to 1,137 pounds. Restoring the load of 1,137 pounds, the deflection became 0.5131 inch, and the original load of 1,233 pounds brought it to 0.5456 inch. The bar was now held at this deflection and the set gradually took place, the effort falling in fifteen minutes to 1,133 pounds—four per cent. more than at the first observation—in twenty-two minutes to 1,093, in forty-six minutes to 1,063, in sixty-three minutes to 1,043, in 91½ minutes to 1,003, and in 118 minutes to 911 pounds; at which last strain the bar broke three minutes later, the deflection remaining unchanged up to the instant of fracture. This remarkable case has already been referred to in an earlier paper, when treating of the effect of time in producing variation of resistance and of the elastic limit.

Nos. 561, copper-tin, and 612, copper-zinc, were compositions which behaved quite similarly to the iron bar at its first trial, the set apparently becoming nearly complete in the first after one hour, and in the second after three or four hours.

In all of these metals, the set and the loss of effort to resume the original form, were phenomena requiring time for their progress, and in all, except in the case of No. 599—which was loaded heavily—the change gradually became less and less rapid, tending constantly toward a maximum.

So far as the observation of the writer has yet extended, the latter is always the case under light loads. As heavier loads are added, and the maximum resistance of the material is approached, the change continues to progress longer, and, as in the case of the brass above described, it may progress so far as to produce rupture, when the load becomes heavy, if the metal does not belong to the "iron-class." The brass broke under a stress 25 per cent. less than it had actually sustained previously.

There is no evidence that iron or steel ever exhibits this treacherous and exceedingly dangerous behaviour; but, on the contrary, it seems always to carry a load, once borne, however near the maximum it may be. This difference is here, quite as marked as in the experiments previously reported, upon the elevation and the depression of the elastic limit by strain; and no one can fail to note the value in construction of this quality of that metal which is the chief reliance of the engineer in nearly every branch of his art. These principles will find numberless applications in the practice of every member of the profession.

The records are herewith presented, and the curves representing them, shown in Plate I:

RECORDS OF EXPERIMENTS ON RATE OF SET OR DECREASE OF RESISTANCE AND INCREASE OF SET OF METALS WITH TIME.

Bars 1 inch square, 22 inches between supports.

Time.	Load.	Loss of Load.	Deflection.	Set.
Minutes	Pounds.	Pounds.	Inches.	Inches.
	No. 648. WROUGHT IRON.			
	First Trial.			
..	1,003	..	0.0995	..
..	3	..	..	0.0049
..	1,003	..	0.1001	..
25	999	4	0.1001	..
100	991	12	0.1001	..
275	987	16	0.1001	..
320	987	16	0.1001	..
320	3	..	..	0.007
322	987	..	0.991	..
322	1,003	..	0.1003	..
..	2,720	..	2.64	..
	Second Trial.			
..	1,003	..	2.2548	..
..	3	..	..	0.1091
..	1,603	..	0.287	..
1	1,521	82	0.287	..
2	1,493	110	0.287	..
3	1,483	120	0.287	..
23	1,463	140	0.287	..
53	1,461	142	0.287	..
133	1,459	144	0.287	..
193	1,457	146	0.287	..
363	1,457	146	0.287	..
363	3	..	..	0.1481
..	1,457	..	0.2863	..
..	1,603	..	0.3016	..
..	2,720	..	2.64	..
..	..	..	..	..
No. 561.	27.5 PARTS COPPER, 72.5 PARTS TIN.			
..	160	..	0.0696	..
..	5	..	..	0.0145
..	160	..	0.072	..
1	154	6	0.072	..
3	150	10	0.072	..
2,640	104	56	0.072	..
4,140	100	60	0.072	..
..	5	..	..	0.04
..	100	..	0.0763	..
..	160	..	0.097	..
..	320	..	0.22	Broke.

Time.	Load.	Loss of Load.	Deflection.	Set.
Minutes	Pounds.	Pounds.	Inches.	Inches.
No. 599.	10 PARTS COPPER, 90 PARTS ZINC.			
..	1,233	..	0.5209	..
15	1,137	..	0.5209	..
..	3	..	..	0.2736
..	1,137	..	0.5131	..
..	1,233	..	0.5456	..
15	1,133	100	0.5456	..
28	1,093	140	0.5456	..
40	1,070	163	0.5456	..
46	1,063	170	0.5456	..
63	1,043	190	0.5456	..
77.5	1,023	210	0.5456	..
91.5	1,003	230	0.5456	..
96.5	993	240	0.5456	..
118	911	322	..	..
121	911	326	..	Broke.
No. 612.	47.5 PARTS COPPER, 52.5 PARTS ZINC.			
..	800	..	0.3332	..
..	3	..	..	0.1478
..	800	..	0.3366	..
5	790	10	0.3366	..
25	778	22	0.3366	..
120	766	34	0.3366	..
480	756	44	0.3366	..
1,320	751	49	0.3366	..
..	3	..	..	0.1688
..	751	..	0.3364	..
..	800	..	0.349	..
..	1,100	..	..	Broke.
No. 655.	QUEENSLAND TIN.			
..	100	..	0.2109	..
..	3	..	..	0.1753
..	100	..	0.2415	..
1	70	30	0.2415	..
3	62	38	0.2415	..
8	56	44	0.2415	..
..	100	..	0.3033	..
..	150	..	Bent rapidly.	

SECTION II.—THE OBSERVED INCREASE OF DEFLECTION UNDER STATIC LOAD.—In the preceding section, the writer presented results of an investigation made to determine the time required to produce "set" in metals belonging to the two typical classes, which exhibit—the one an exaltation and the other a depression of the elastic limit under strain.

The experiments there described, were made by means of a testing machine in which the test piece could be securely held at a given degree of distortion, and its effort to recover its form measured at intervals, until the progressive loss of effort could no longer be detected, and until it was thus indicated that set had become complete.

The deductions were :

That in metals of all classes, under light loads, this decrease of effort and rate of set become less and less noticeable until, after some time, no further change can be observed, and the set is permanent:

That in metals of the "tin class," or those which had been found to exhibit a depression of the elastic limit with strain, a heavy load, *i. e.*, a load considerably exceeding the proof-strain, the loss of effort continued until, before the set had become complete, the test piece yielded entirely:

And that in the metals of the "iron-class," or those exhibiting an elevation of elastic limit by strain, the set became a maximum and permanent and the test-piece remained unbroken, no matter how near the maximum load the strain may have been.

The experiments here described were conducted with the same object as those above referred to. In these experiments, however, the load, instead of the distortion, was made constant, and deflection was allowed to progress, its rate being observed, until the test-piece either broke under the load or rapidly yielded, or until a permanent set was produced. It will be seen that the results of these experiments are in striking accordance with those conducted in the manner previously described; they exhibit the fact of a gradually changing rate of set for the several cases of light or heavy loads, and illustrate the striking and important distinctions between the two classes of metals even more plainly than the preceding. The accompanying record and the strain-diagrams, (Plate II), which are its graphical representation, will assist the reader in comprehending the method of research and its results. All test-pieces were of one-inch square section, and loaded at the middle. The bearings were 22 inches apart.

No. 651 was of wrought iron from the same bar with No. 648, already described. This specimen subsequently gave way under a load of 2,587 pounds. Its rate of set was determined at about 60 per cent. of its ultimate resistance, or at 1,600 pounds. Its deflection, starting at 0.489 inch, increased in the first minute 0.1047, in the second minute 0.026, in the third minute 0.0125, in the fourth minute 0.0088, in the fifth minute 0.0063, and in the sixth minute 0.0031 inch; the total deflection being 0.5937, 0.6197, 0.6322, 0.641, 0.6473, and 0.6504 inch. In the succeeding 10 minutes the deflection only increased 0.0094 inch, or to 0.6598 inch, and remained at that point without increasing so much as 0.0001 inch, although the load was allowed to remain 344 minutes untouched. The bar had evidently taken a permanent set, and it seems to the writer probable, that it would have remained at that deflection indefinitely, and have been perfectly free from liability to fracture for any length of time.

This bar finally yielded completely, under a load of 2,589 pounds, deflecting 4.67 inches.

No. 479 was a copper bar containing $3\frac{3}{4}$ per cent. of tin. Its behaviour may be taken as typical of that of the whole "tin-class" of metals, as the preceding illustrates the behavior of the "iron-class" under heavy loads. It was subjected to two trials, the one under a load of 700 and the other of 1,000 pounds, and broke under the latter load, after having sustained it $1\frac{1}{4}$ hours. The behavior of this bar will be considered especially interesting, if its record and strain-diagram are compared with those of No. 599, previously given, which latter specimen broke after 121 minutes when held at a constant deflection of 0.5456 inch; its resistance gradually falling from an initial amount of 1,233 pounds, to 911 pounds at the instant before breaking.

This bar, No. 479, was loaded with 700 pounds "dead weight," and at once deflected 0.441 inch. The deflection increased 0.118 inch in the first 5 minutes, 0.024 in the second 5 minutes, 0.018 in the second 10 minutes, 0.17 in the fourth, 0.012 in the fifth, and 0.008 inch in the sixth 10 minute-period, the total set increasing from 0.441 to 0.65 inch. The record and the strain-diagram, (Plate II), show that, at the termination of this trial, the deflection was regularly increasing. The load was then removed and the set was found to be 0.524 inch, the bar springing back 0.126 inch on removal of the weight.

The bar was again loaded with 1,000 pounds. The first deflection which could be caught and measured, was 3.118 inches and the increase at first followed

ways in which there is any danger from fire.

One is where the powder is completely inclosed in some strong vessel and set on fire, as already explained. The vessel must be much stronger than any in which the powders are ever transported, and must be so tight that the gases can not escape as fast as they form.

There is no practical danger from this source in transportation.

The other way is in roasting, toasting and baking the powder, when frozen.

When frozen cartridges are put into a hot oven, upon stoves or boilers, or in kettles over a fire, or placed before a hot fire and allowed to remain long enough to thaw, and become so hot as to smoke, the result is that in about nineteen cases out of twenty the powders take fire and burn up, or all the nitroglycerin is evaporated, and they are ruined.

But in the twentieth case there will be an explosion. If all the powder is equally exposed to the heat, as the evaporation commences long before the exploding point is reached, the powder is weakened, and the explosion is correspondingly weak—often a mere pop or puff.

But if there is a large quantity of powder, some of which is thus heated, and the balance left unaffected, the explosion may extend to the full strength powder, and be violent accordingly.

But as this mode of heating frozen powder cannot take place during transportation, it does not belong in the discussion, and is only mentioned for the sake of having the review complete.

SPONTANEOUS DECOMPOSITION.

Liquid nitroglycerin, as already stated, unless completely freed from the acids employed in its manufacture, is liable to voluntary decomposition, especially in warm times and places, resulting in most cases in combustion, but under favorable circumstances, in explosion.

By the oversight, or carelessness of a workman, this danger may be incurred. But this instability is not found in the powder. No instance of such decomposition has ever been known.

Besides, it has been shown by repeated experiments that nitroglycerin impregnated with acids, and which will decompose while in the liquid form, may be made into powder, and subjected to all the conditions most favorable for decomposition, without that result.

The powder form, that is, the solid form, seems to be an effectual bar to the peculiar chemical action requisite for spontaneous decomposition.

EXPERIMENTAL TESTS.

Having considered the structure of the high explosive powders and the circumstances which affect them, let us briefly review some of the actual experiments touching their safety.

We believe there has never been any question but these powders are abundantly safe from explosion by fire. The following are common tests which may be repeated by any one at any time.

If set on fire in piles, large or small, either loose or in cartridges, it burns up rapidly like chaff when loose, but slowly like rosin, tar or sulphur, when in cartridges.

Set fire to one end of a cartridge and it burns much like a Roman candle, without the pop but with less speed.

When partly burned it may be extinguished by water or the foot.

As packed for transportation in boxes of inch boards strongly nailed and set on fire by a fuse through a gimlet hole, its gases spring the boards apart and the flame issues. A box of one-hundred pounds is burned in from two to five minutes, according to the composition of the powder. Those who know the powder stand upon the box while it is burning.

Poured upon a red hot iron it burns.

A red hot iron thrust into it sets it on fire.

The Steamer "Meteor" took fire from its furnace and burned to the water's edge on Lake Erie consuming 8,000 pounds of Giant Powder on board without explosion.

The only source from which the enemies of these powders profess to apprehend danger during transportation, is percussion, concussion, shocks and blows.

The following experiments have been repeated many times:

A minute quantity is placed on an anvil and struck with a hammer. A snapping sound is produced like the breaking of a stick, and the particles be-

tween the points in contact are exploded and no more—the balance is scattered.

If the quantity be increased, and the blows repeated until the powder is made solid, a greater quantity can be exploded; but if the quantity is so large that the blows are deadened and the percussive action prevented there will be no explosion.

A cartridge of powder laid upon a rail and passed over by cars will be cut in two, and the small amount between the wheel and rail will be exploded, if not by the first wheel, by subsequent ones, but the balance of the cartridge will not explode.

Pounding it upon wood will not explode it.

A box of it thrown from any height upon rocks will be broken in pieces, but no explosion can be thus caused, nor by dropping heavy weights upon it. A box of it between car buffers, meeting at a speed of sixty miles an hour, will not be exploded.

If special preparations are made it can be exploded in quantity by percussion.

By putting it into an iron cylinder, fitted with a piston, it can be exploded by blows on the piston sufficiently numerous and heavy. But, of course, this cannot occur in transportation.

A car loaded with iron rails at full speed collided with a car of powder, and the rails were driven through the boxes and powder without exploding it.

ACTUAL EXPERIENCE.

The manufacture of dynamite commenced in 1867 and the trade in it has gradually increased until it now amounts in this country and Europe to about 15,000,000 pounds per annum.

Its transportation has taken place by all the ordinary means, on vessels and vehicles of every description. It has met with such kinds of treatment as would naturally befall an article supposed to be safe in every respect and handled roughly. Nevertheless, there has never been an accidental explosion from any cause while being transported either in this country or abroad, except the Dualin at Worcester already mentioned.

There have occurred several explosions supposed, at least to some extent, to have been due to these powders but the supposition is unwarranted.

To avoid all misunderstanding we will enumerate them. The most notable case was the Bremerhaven explosion. This was not of dynamite proper but of a compound resembling it, called Lithofracteur. However, the powder was not to blame, as the explosion was caused by an exploder placed in the powder by design, and fired by clock work.

The Bergen Hill explosion was of what is known as Rend Rock powder, and was caused by exploders applied by workmen during a strike.

An explosion of dynamite took place in San Francisco while being prepared for a large blast under water. The loose powder which was being packed into cartridges was set on fire from the pipe of a workman, and before it could be extinguished the fire reached exploders in other cartridges near by and of course, caused their explosion.

The explosion last year at Drakesville was of liquid nitroglycerin, while being weighed and transferred.

The fact that it occurred at a dynamite factory led to the report that it was dynamite.

AUTHORITY.

As would be naturally expected with a substance so novel, powerful and useful as these powders, they have received careful attention and study, and been subjected to a variety of tests in all quarters with reference to their safety.

Men of science; experts in explosives; men in charge of works requiring their use; committees on behalf of transportation companies; military gentlemen and government commissions, and numerous others have examined the subject, and reported upon it,—some of them elaborately.

It would be inappropriate to reproduce these reports here or even extracts or their substance in detail. They are before the public and can be referred to by those who desire to do so.

We must content ourselves with a brief summary of the conclusions from all of them, to wit:

1. These powders are the most powerful of all the disrupting agents now in general use.

2. When properly made and economically transported they are, for hard rock work and sub-aqueous work, by far the most economical explosives in use.

3. They are the safest of all explosives both in transportation and use, many times safer than gunpowder, and when properly made and a few simple precautions taken, are as practically safe for transportation as if they were wholly inexplosive.

4. There is no good reason why, under proper regulations, they should not be transported in freight conveyances as freely as any ordinary merchandise.

STATE OF PRESENT LEGISLATION.

The congressional enactments now in force on this subject were passed July 3, 1866, before dynamite was invented, and are found in secs. 4,278, 4,279, 4,280, 5,353, 5,354 and 5,355 of the Revised Statutes.

Their design was to regulate the transportation of nitroglycerin, which is a liquid, but by accident they were so drawn as apparently to embrace all compounds containing nitroglycerin. Nevertheless, in a case before the U. S. District Court for the Northern District of New York, it was settled that dynamite or Giant Powder, was not within the law. But there is no certainty that a like result would be reached in all cases.

This uncertainty is itself a serious objection, and ought to be removed.

If the law applies to the powders they cannot be conveyed on public conveyances unless packed in metallic cases, and these cases be surrounded by plaster of Paris and their outside be conspicuously marked, "Nitroglycerin—Dangerous."

From what has gone before it is plain that each one of these requirements is the exact reverse of what it ought to be.

As to the metallic cases, they were necessary for liquid nitroglycerin but are not so for the the powders; besides, as already shown, they are more dangerous than wood or paper.

As to the plaster of Paris, its purpose was to absorb the nitroglycerin in case of leakage from the metallic vessel. But in making these powders this absorption has already taken place fully, and to guard against leakage from a dry thing is simple folly.

It will be noticed that this law recognizes the dynamite principle as the basis of safety.

As to marking the powders "*Nitroglycerin—Dangerous,*" it would be a double falsehood.

They are not nitroglycerin, nor are they dangerous, any more than wet gunpowder is gunpowder and dangerous.

This death's head and cross bones sign, marked conspicuously on a box of the powders, or on a dry goods box would prevent its reaching its destination as effectually as if marked "*Death to him that touches.*"

The practical result of these absurd and useless requirements has been their own *felo de se.* Not a pound of these powders has ever been carried in metallic cases, or surrounded with plaster of Paris, or marked "*Nitroglycerin—dangerous.*"

They are carried either by private conveyance at immense extra expense, or on public freight conveyances without being packed or marked as the law requires, or smuggled on passenger conveyances and by express under false names.

This shows that the law, so far as it affects these powders is worse than useless. But, on the other hand, if it does not apply to these powders there is no law of Congress that does, and they are free to be carried at pleasure on passenger conveyances, and in the midst of dense populations dripping with nitroglycerin—a state of things too alarming to require further comment.

This subject has already been considered by the Legislatures of several States, some of which have passed laws upon it substantially like the congressional one, and others have not, mainly because of their limited application. State laws are limited in their operation to such cases of transport as begin, continue and end within the State, while almost all transportations of these powders extend beyond a single State, and are, therefore, exclusively within congressional jurisdiction.

The great vice of the laws now in force is that they recognize no difference between liquid nitroglycerin and these powders.

Under the law as it now stands, the liquid can go where the powder can, and the powder cannot go where the liquid cannot.

Such a law as this ought not to stand another day.

The practical world has, for ten years

past, thoroughly recognized a difference —why should not law makers do so?

If the claims now made against nitroglycerin are true, ought it longer to be permitted to be transported in the dangerous manner laid down in the law?

And if the claims made in behalf of the powders are true, ought they to be classed with liquid nitroglycerin?

And if the law does not apply to the powders, and they are free to be carried on passenger conveyances, even when in a leaky and dangerous condition, ought not a new law against this to be enacted forthwith?

There never was a case where the necessity for immediate and intelligent action was more apparent and pressing than in this.

If, what we have written is true, our conclusions are undoubtedly just and proper; if, what we have written is not true, it can be easily disproved; but until it is so disproved the duty of at least a careful examination of the subject is palpably clear.

GENERAL FEATURES OF A PROPER LAW.

We now come to the practical question as to what should be the nature of the regulations on this subject.

They are few, simple and obvious:

First: As liquid nitroglycerin is known to be dangerous—so much so that no prudent manufacturer asks or attempts to transport it—and especially as there is now no necessity for it—its transportation on public conveyances and by common carriers should be absolutely prohibited, not only on passenger conveyances but also on freight conveyances.

Second: Leaky powders ought to be classed with liquid nitroglycerin, and wholly debarred from transportation in like manner.

Third: The dry and safe powders ought to be carried on all public freight conveyances.

The test of safety should be this, that when at the highest temperature to which they are to be exposed during their transit, they will not leak or part with any portion of the nitroglycerin which they contain when so placed that it can drain therefrom if it will.

Fourth: These powders, even when dry and safe, ought not to be transported on passenger conveyances. The ground on which this regulation ought to be made is that the powders are *explosive* and the persons of passengers ought not to be subjected to *any* risk however remote from this source.

However safe the powders there would be *fear*. To the timid and ignorant this fear would amount to intense pain. Passengers ought not to be thus tormented.

Besides there is no necessity for transport on passenger conveyances if one by freight is open.

Fifth: The powders ought not to be packed in metallic cases. The fact that metallic cases add almost nothing to the danger and that no accident has ever occurred from this cause, is no good ground against the regulation.

Sixth: Large percussion caps or exploders, or other things whose explosion by fire will explode the powders ought not to be transported in conjunction with them. If the Worcester explosion was caused by the exploders on board it is a case illustrating the propriety of this regulation.

Seventh: Each separate package ought to be marked on the outside with the name of the contents so as to be legible to those who handle it.

These are all the regulations which an experience of ten years has suggested as necessary.

Some of these are almost useless but none of them absurd like most of those now in force. They are suggested out of abundant caution and because of the extremely sensitive state of the public mind on the subject of nitroglycerin.

Under these regulations these powders will be a hundred fold less dangerous than gunpowder and we may confidently hope and expect there will be no accidents whatever in their transport.

DRAINING OF THE ZUYDER ZEE.—A dam 40 kilometers (24¾ miles) long, 50 meters broad at its base and 1½ meters above the usual level of high water, is to be carried across the gulf. Upon this will be erected pumping engines capable of discharging 1,716,000,000 gallons a day. Estimating the average depth of the gulf at about 4½ meters, it will make steady pumping for 16 years to empty the enclosure.

THE RATE OF SET OF METALS SUBJECTED TO STRAIN FOR CONSIDERABLE PERIODS OF TIME.

By Prof. ROBERT H. THURSTON.

Transactions of American Society of Civil Engineers.

Section I.—On the Observed Decrease of Resistance at a Fixed Distortion.—The writer has, in a preceding paper shown, by reference to experimental researches in which he had then engaged, that some classes of metals, as ordinary iron and steel, when subjected to strain and distortion by a force exceeding the resistance of the material within the elastic limit, take a set and are stiffened by that act, and exhibit an exaltation of the elastic limit. It was also shown that other classes, like tin, and similarly viscous and ductile materials, exhibit flow and a depression of their limits of elasticity when similarly treated. It was further shown, that the former class when subjected to loads, even approaching their ultimate strength, took a certain set and remained apparently indefinitely without further distortion; while the second class, under very moderate loads, frequently exhibit a gradual yielding, a progressive distortion, until fracture took place, sometimes under stresses which were but a fraction of those which were found required to break such metals quickly, and when time was not allowed for flow to occur. It was noted that increase of rapidity of distortion and fracture produced increase of resistance in the latter, or "tin-class," and decrease of resisting power in the first, or "iron-class," and *vice versa*.

The writer has since instituted experiments upon metals of both classes to determine how rapidly set, in each class, took place; the earlier experiment just referred to, having confirmed a suspicion long existing among engineers and experimentalists, that the phenomenon was a molecular change, as well as of the mass, and that time was required for its complete development. Prof. Norton has also shown by experiment that this set is partially temporary, the bar relieving itself of distortion in some degree, on removal of the load. Both that experimenter and the writer had detected some peculiar variations of form during this recovery, and the experiments of the latter, as detailed in the preceding paper, exhibited at times a gradual recovery of straightening power in a confined and flexed bar. The following will be found interesting, and perhaps, important, as showing how these molecular changes progress.

Bars were prepared of square section, 1 inch in breadth and depth, and 22 inches in length, between bearings. They were flexed in a machine for testing the resistance of materials to transverse stress, as described in the preceding paper and the load and deflection carefully measured. As the bars were retained at a constant deflection, their effort to resume their original form gradually decreased, and the amount of this effort was, from time to time, noted. When this effort or resistance had become considerably decreased, the bar was released, and the set measured. This operation was repeated with each, until the law of decrease of elastic resistance was detected. Curves were constructed, illustrating graphically this aw, and exhibiting in more satisfactorily and more plainly than the tubular record.

The following is the record for the bars of iron, of tin, and of two alloys. The iron bar No. 648, was subjected to a load 1,003 pounds, somewhat less than one-half its maximum, and its deflection was found to be 0.0995 inch. Removing the load, the set was 0.0049 inch. Restoring the load (1,000 pounds, + 3 pounds due to the weight of the bar), the deflection was 0.1001 inch, and the bar was held at this deflection and the decrease of resistance observed. In 25 minutes, it had become 999 pounds; in 1 hour 40 minutes, 991 pounds; in 4 hours 35 minutes, 987 pounds, and in 5 hours 20 minutes, 987 pounds. The set was then found to be 0.007 inch under the weight of the bar itself.

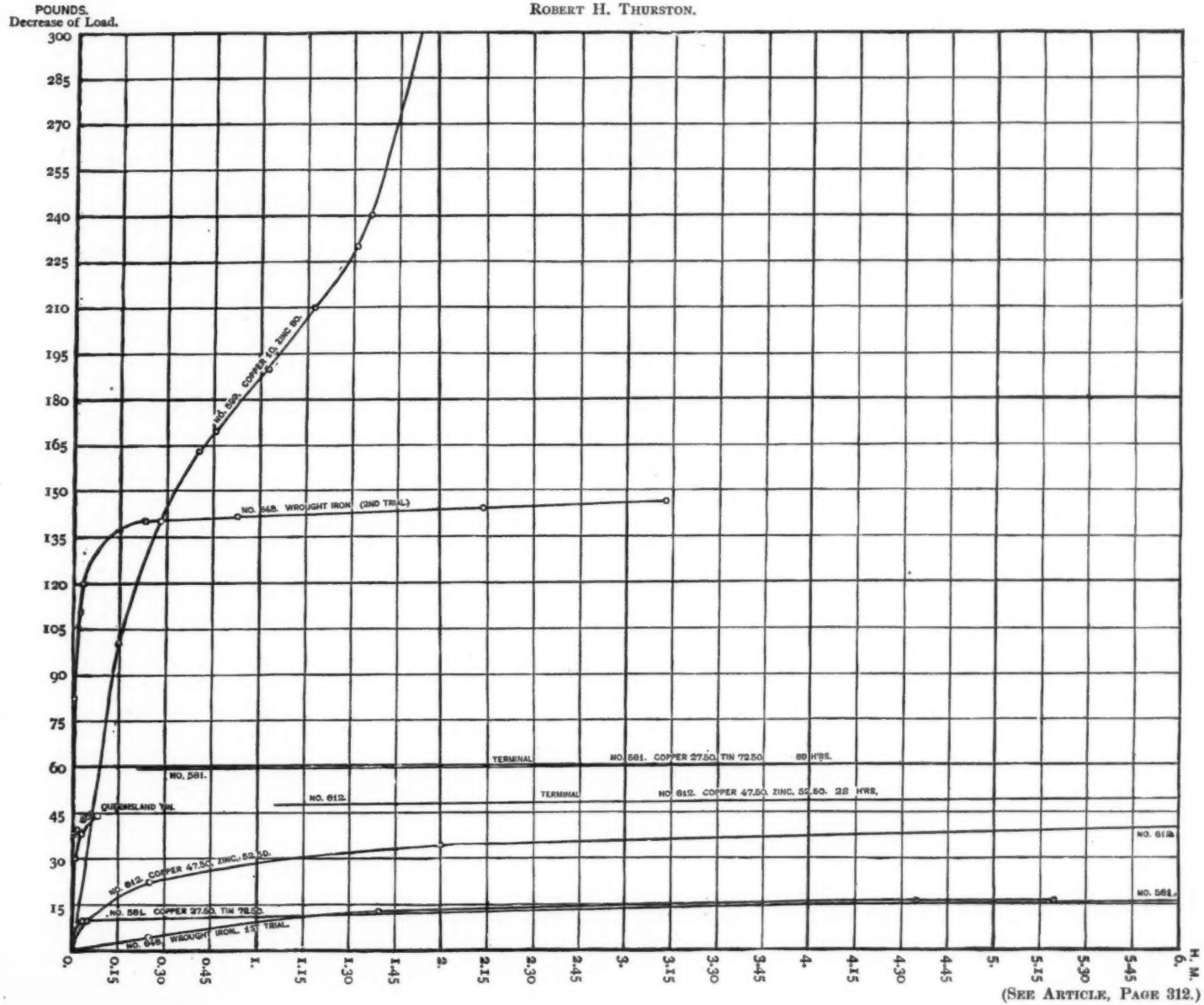
Restoring the last observed load, the deflection was 0.0991 inch, and the origi-

PLATE I.

DECREASE OF RESISTANCE WITH TIME IN TRANSVERSE TESTS OF BARS OF METAL.

RATE OF SET OF BARS, 1 INCH SQUARE, 22 INCHES BETWEEN SUPPORTS

ROBERT H. THURSTON.



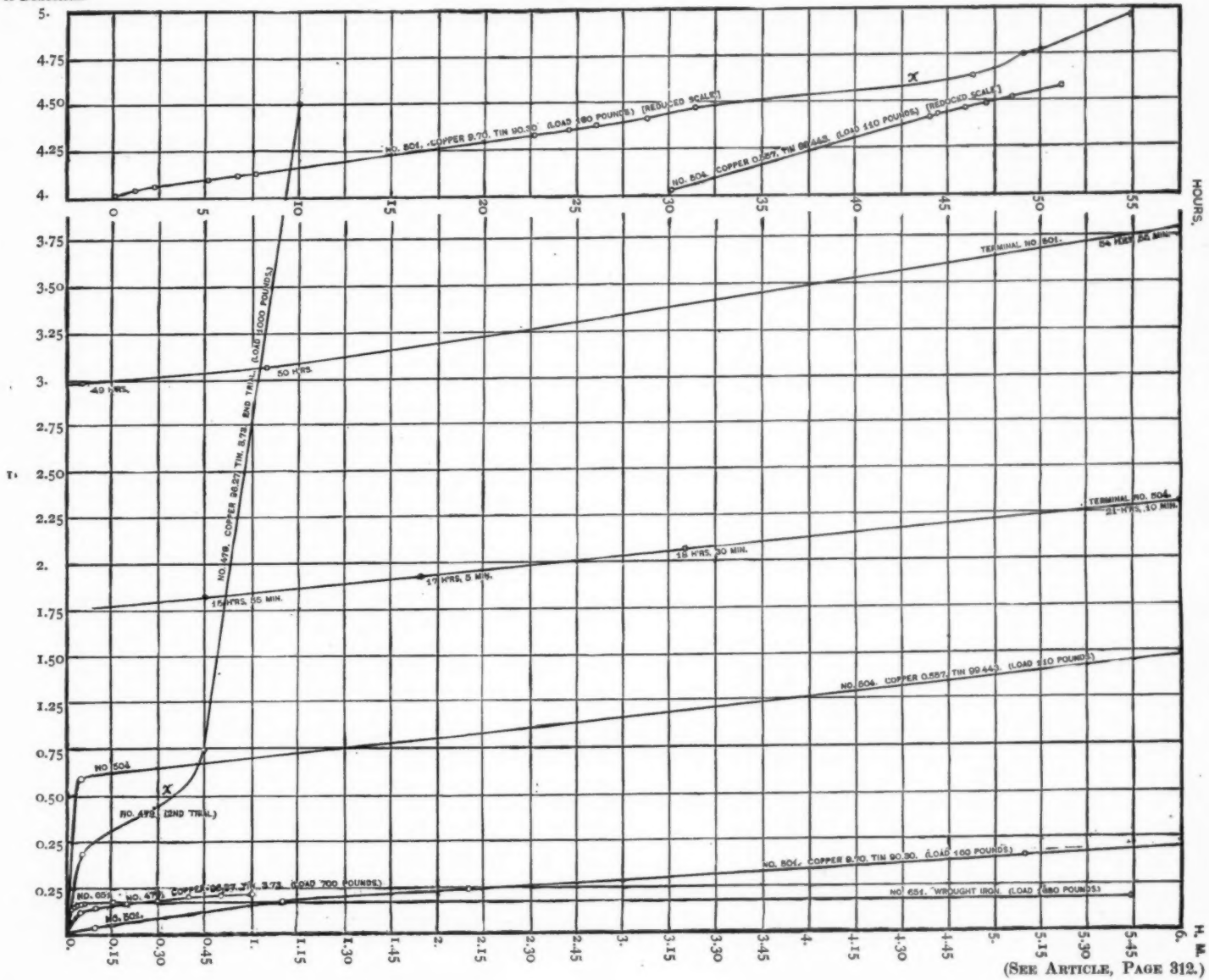
(SEE ARTICLE, PAGE 312.)

PLATE II.

INCREASE OF DEFLECTION WITH TIME IN TRANSVERSE TESTS OF BARS OF METAL.

RATE OF SET OF BARS, 1 INCH SQUARE 22 INCHES, BETWEEN SUPPORTS.

ROBERT H. THURSTON.


INCHES of Deflection.
HOURS.
H. M.
NO. 504. COPPER 9.70. TIN 90.30. (LOAD 100 POUNDS) (REDUCED SCALE)
NO. 504. COPPER 0.557. TIN 99.443. (LOAD 110 POUNDS) (REDUCED SCALE)
TERMINAL NO. 504.
54 HRS. 24 MIN.
49 HRS.
50 HRS.
NO. 498. COPPER 90.27. TIN. 9.73. END TRIAL. (LOAD 1000 POUNDS.)
TERMINAL NO. 504.
21 HRS. 10 MIN.
18 HRS. 30 MIN.
17 HRS. 5 MIN.
15 HRS. 55 MIN.
NO. 504. COPPER 0.557. TIN 99.443. (LOAD 110 POUNDS)
NO 504
NO. 498. (END TRIAL.)
NO. 504. COPPER 9.70. TIN 90.30. (LOAD 100 POUNDS)
NO. 654. WROUGHT IRON. (LOAD 680 POUNDS)
NO. 654.
NO. 498. COPPER 90.27. TIN. 9.73. (LOAD 700 POUNDS)
NO. 504.

(SEE ARTICLE, PAGE 312.)

nal load of 1,003 pounds increased it to 0.1003 inch.

A second trial of the same bar under a load of 1,603 pounds gave a deflection of 0.2548 inches, and a set, on removal, of 0.1091 inch. Restoring the load, the deflection became 0.287 inch, and the resistance to flexion decreased in 6 hours 3 minutes, from 1,603 to 1,457 pounds, at which latter time the set was found to be 0.1451 inch. Restoring the load of 1,457 pounds, the deflection was 0.2863 inch, and the original load, 1,603 pounds, being brought upon it, its deflection increased to 0.3016 inch, an increase nearly twenty per cent. above the original deflection.

In the first trial the loss of stiffness, as measured by the decrease of effort to straighten itself, and which is here taken to measure the *rate of set*, is seen to have been nearly proportional to the time at first, becoming constant after 4½ hours. On the second trial, after a considerable set, produced by a heavy load, the set became constant after about one hour, and so remained to the end of the trial.

No. 655 was a bar of Queensland tin, received from the Commissioner of that country at the Centennial Exhibition, and which was found to be remarkably pure. A load of 100 pounds gave a deflection of 0.2109 inch, and produced a set of 0.1753 inch. The same load restored, deflected the bar 0.2415 inch, which deflection being retained, the effort to regain the original shape decreased in one minute from 100 to 70 pounds, in three minutes to 62, and in eight minutes to 56 pounds. The original load of 100 pounds then brought the deflection to 0.3033 inch, nearly fifty per cent. more than at first.

A bar, No. 599, of copper-zinc alloy similarly tested, deflected 0.5209 inch under 1,233 pounds, and took a set of 0.2736 inch after being held at that deflection fifteen minutes, the effort falling, meantime to 1,137 pounds. Restoring the load of 1,137 pounds, the deflection became 0.5131 inch, and the original load of 1,233 pounds brought it to 0.5456 inch. The bar was now held at this deflection and the set gradually took place, the effort falling in fifteen minutes to 1,133 pounds—four per cent. more than at the first observation—in twenty-two minutes to 1,093, in forty-six minutes to 1,063, in sixty-three minutes to 1,043, in 91½ minutes to 1,003, and in 118 minutes to 911 pounds; at which last strain the bar broke three minutes later, the deflection remaining unchanged up to the instant of fracture. This remarkable case has already been referred to in an earlier paper, when treating of the effect of time in producing variation of resistance and of the elastic limit.

Nos. 561, copper-tin, and 612, copper-zinc, were compositions which behaved quite similarly to the iron bar at its first trial, the set apparently becoming nearly complete in the first after one hour, and in the second after three or four hours.

In all of these metals, the set and the loss of effort to resume the original form, were phenomena requiring time for their progress, and in all, except in the case of No. 599—which was loaded heavily—the change gradually became less and less rapid, tending constantly toward a maximum.

So far as the observation of the writer has yet extended, the latter is always the case under light loads. As heavier loads are added, and the maximum resistance of the material is approached, the change continues to progress longer, and, as in the case of the brass above described, it may progress so far as to produce rupture, when the load becomes heavy, if the metal does not belong to the "iron-class." The brass broke under a stress 25 per cent. less than it had actually sustained previously.

There is no evidence that iron or steel ever exhibits this treacherous and exceedingly dangerous behaviour; but, on the contrary, it seems always to carry a load, once borne, however near the maximum it may be. This difference is here, quite as marked as in the experiments previously reported, upon the elevation and the depression of the elastic limit by strain; and no one can fail to note the value in construction of this quality of that metal which is the chief reliance of the engineer in nearly every branch of his art. These principles will find numberless applications in the practice of every member of the profession.

The records are herewith presented, and the curves representing them, shown in Plate I:

RECORDS OF EXPERIMENTS ON RATE OF SET OR DECREASE OF RESISTANCE AND INCREASE OF SET OF METALS WITH TIME.

Bars 1 inch square, 22 inches between supports.

Time.	Load.	Loss of Load.	Deflection.	Set.
Minutes	Pounds.	Pounds.	Inches.	Inches.
	No. 648. WROUGHT IRON.			
	First Trial.			
..	1,003	..	0.0995	..
..	3	..	..	0.0049
..	1,003	..	0.1001	..
25	999	4	0.1001	..
100	991	12	0.1001	..
275	987	16	0.1001	..
320	987	16	0.1001	..
320	3	..	..	0.007
322	987	..	0.991	..
322	1,003	..	0.1003	..
..	2,720	..	2.64	..
	Second Trial.			
..	1,003	..	2.2548	..
..	3	..	..	0.1091
..	1,603	..	0.287	..
1	1,521	82	0.287	..
2	1,493	110	0.287	..
13	1,483	120	0.287	..
23	1,463	140	0.287	..
53	1,461	142	0.287	..
133	1,459	144	0.287	..
193	1,457	146	0.287	..
363	1,457	146	0.287	..
363	3	..	..	0.1481
..	1,457	..	0.2863	..
..	1,603	..	0.3016	..
..	2,720	..	2.64	..
..	..	..	..	..
No. 561.	27.5 PARTS COPPER, 72.5 PARTS TIN.			
..	160	..	0.0696	..
..	5	..	..	0.0145
..	160	..	0.072	..
1	154	6	0.072	..
3	150	10	0.072	..
2,640	104	56	0.072	..
4,140	100	60	0.072	..
..	5	..	..	0.04
..	100	..	0.0763	..
..	160	..	0.097	..
..	320	..	0.22	Broke.

Time.	Load.	Loss of Load.	Deflection.	Set.
Minutes	Pounds.	Pounds.	Inches.	Inches.
No. 599.	10 PARTS COPPER, 90 PARTS ZINC.			
..	1,233	..	0.5209	..
15	1,137	..	0.5209	..
..	3	..	..	0.2736
..	1,137	..	0.5131	..
..	1,233	..	0.5456	..
15	1,133	100	0.5456	..
28	1,093	140	0.5456	..
40	1,070	163	0.5456	..
46	1,063	170	0.5456	..
63	1,043	190	0.5456	..
77.5	1,023	210	0.5456	..
91.5	1,003	230	0.5456	..
96.5	993	240	0.5456	..
118	911	322	..	..
121	911	326	..	Broke.
No. 612.	47.5 PARTS COPPER, 52.5 PARTS ZINC.			
..	800	..	0.3332	..
..	3	..	..	0.1478
..	800	..	0.3366	..
5	790	10	0.3366	..
25	778	22	0.3366	..
120	766	34	0.3366	..
480	756	44	0.3366	..
1,320	751	49	0.3366	..
..	3	..	..	0.1688
..	751	..	0.3364	..
..	800	..	0.349	..
..	1,100	..	..	Broke.
No. 655.	QUEENSLAND TIN.			
..	100	..	0.2109	..
..	3	..	..	0.1753
..	100	..	0.2415	..
1	70	30	0.2415	..
3	62	38	0.2415	..
8	56	44	0.2415	..
..	100	..	0.3033	..
..	150	..	Bent rapidly.	

SECTION II.—THE OBSERVED INCREASE OF DEFLECTION UNDER STATIC LOAD.—In the preceding section, the writer presented results of an investigation made to determine the time required to produce "set" in metals belonging to the two typical classes, which exhibit—the one an exaltation and the other a depression of the elastic limit under strain.

The experiments there described, were made by means of a testing machine in which the test piece could be securely held at a given degree of distortion, and its effort to recover its form measured at intervals, until the progressive loss of effort could no longer be detected, and until it was thus indicated that set had become complete.

The deductions were :

That in metals of all classes, under light loads, this decrease of effort and rate of set become less and less noticeable until, after some time, no further change can be observed, and the set is permanent:

That in metals of the "tin class," or those which had been found to exhibit a depression of the elastic limit with strain, a heavy load, *i. e.*, a load considerably exceeding the proof-strain, the loss of effort continued until, before the set had become complete, the test piece yielded entirely:

And that in the metals of the "iron-class," or those exhibiting an elevation of elastic limit by strain, the set became a maximum and permanent and the test-piece remained unbroken, no matter how near the maximum load the strain may have been.

The experiments here described were conducted with the same object as those above referred to. In these experiments, however, the load, instead of the distortion, was made constant, and deflection was allowed to progress, its rate being observed, until the test-piece either broke under the load or rapidly yielded, or until a permanent set was produced. It will be seen that the results of these experiments are in striking accordance with those conducted in the manner previously described; they exhibit the fact of a gradually changing rate of set for the several cases of light or heavy loads, and illustrate the striking and important distinctions between the two classes of metals even more plainly than the preceding. The accompanying record and the strain-diagrams, (Plate II), which are its graphical representation, will assist the reader in comprehending the method of research and its results. All test-pieces were of one-inch square section, and loaded at the middle. The bearings were 22 inches apart.

No. 651 was of wrought iron from the same bar with No. 648, already described. This specimen subsequently gave way under a load of 2,587 pounds. Its rate of set was determined at about 60 per cent. of its ultimate resistance, or at 1,600 pounds. Its deflection, starting at 0.489 inch, increased in the first minute 0.1047, in the second minute 0.026, in the third minute 0.0125, in the fourth minute 0.0088, in the fifth minute 0.0063, and in the sixth minute 0.0031 inch; the total deflection being 0.5937, 0.6197, 0.6322, 0.641, 0.6473, and 0.6504 inch. In the succeeding 10 minutes the deflection only increased 0.0094 inch, or to 0.6598 inch, and remained at that point without increasing so much as 0.0001 inch, although the load was allowed to remain 344 minutes untouched. The bar had evidently taken a permanent set, and it seems to the writer probable, that it would have remained at that deflection indefinitely, and have been perfectly free from liability to fracture for any length of time.

This bar finally yielded completely, under a load of 2,589 pounds, deflecting 4.67 inches.

No. 479 was a copper bar containing $3\frac{3}{4}$ per cent. of tin. Its behaviour may be taken as typical of that of the whole "tin-class" of metals, as the preceding illustrates the behavior of the "iron-class" under heavy loads. It was subjected to two trials, the one under a load of 700 and the other of 1,000 pounds, and broke under the latter load, after having sustained it $1\frac{1}{4}$ hours. The behavior of this bar will be considered especially interesting, if its record and strain-diagram are compared with those of No. 599, previously given, which latter specimen broke after 121 minutes when held at a constant deflection of 0.5456 inch; its resistance gradually falling from an initial amount of 1,233 pounds, to 911 pounds at the instant before breaking.

This bar, No. 479, was loaded with 700 pounds "dead weight," and at once deflected 0.441 inch. The deflection increased 0.118 inch in the first 5 minutes, 0.024 in the second 5 minutes, 0.018 in the second 10 minutes, 0.17 in the fourth, 0.012 in the fifth, and 0.008 inch in the sixth 10 minute-period, the total set increasing from 0.441 to 6.65 inch. The record and the strain-diagram, (Plate II), show that, at the termination of this trial, the deflection was regularly increasing. The load was then removed and the set was found to be 0.524 inch, the bar springing back 0.126 inch on removal of the weight.

The bar was again loaded with 1,000 pounds. The first deflection which could be caught and measured, was 3.118 inches and the increase at first followed

the parabolic law noted in the preceding cases, but quickly became accelerated; this sudden change of law is best seen on the strain-diagram. The new rate of increase continued until fracture actually occurred, at the end of 1¼ hours, and at a deflection of 4.506 inches.

This bar was of very different composition from No. 599; it is a member of the "tin-class," however, and it is seen, by examining their records and strain-diagrams, that these specimens, tested under radically different conditions, both illustrate the peculiar characteristics of the class, by similarly exhibiting its treacherous nature.

RECORD OF EXPERIMENTS WITH "DEAD LOADS" TO DETERMINE THE INCREASE OF DEFLECTION WITH TIME, OR RATE OF SET.

Bars, 1 inch square, 22 inches between supports.

Load applied at the middle.

Time.	Deflection.	Increase. Inches.	
Minutes.	Inches.	Difference.	Total.
No. 651. WROUGHT IRON.			
Load, 1,600 pounds.			
0	0.489	....	....
1	0.5937	0.1047	0.1047
2	0.6197	0.026	0.1307
3	0.6322	0.0125	0.1432
4	0.641	0.0088	0.152
5	0.6473	0.0063	0.1583
6	0.6504	0.0031	0.1614
16	0.6598	0.0094	0.1708
344	0.6598	0.0000	0.1708
Maximum load, 2,589 pounds; maximum deflection, 4.67 inches.			
No. 504. 0.557 PARTS COPPER, 99.443 PARTS TIN.			
Load, 110 pounds.			
0	0.323	....	....
5	0.406	0.083	0.083
845	1.945	1.539	1.622
865	2.005	0.059	1.681
895	2.138	0.134	1.815
1,025	2.248	0.11	1.925
1,110	2.378	0.13	2.055
1,270	2.626	0.248	2.303
Maximum load, 130 pounds; maximum deflection, 8.11 inches.			
No. 479. 96.27 PARTS COPPER, 3.73 PARTS TIN.			
Load, 700 pounds.			
0	0.441	....	....
5	0.559	0.118	0.118
10	0.583	0.024	0.142
20	0.601	0.018	0.16
30	0.618	0.017	0.177
40	0.63	0.012	0.189
50	0.642	0.012	0.201
60	0.65	0.008	0.209
Set.	0.524	....	....
Second Trial.—Load, 1,000 pounds.			
0	3.118	....	....
5	3.54	0.422	0.422
15	3.66	0.12	0.542
45	4.102	0.442	0.984
75	7.634	3.522	4.506
Bar broke under 1,000 pounds.			
No. 501. 9.7 PARTS COPPER, 90.3 PARTS TIN.			
Load, 160 pounds.			
0	1.294	....	....
10	1.319	0.025	0.025
70	1.463	0.144	0.169
130	1.53	0.067	0.236
310	1.691	0.161	0.397
400	1.766	0.075	0.472
460	1.811	0.045	0.517
1,360	2.534	0.723	1.24
1,475	2.697	0.163	1.403
1,565	2.782	0.085	1.488
1,730	2.938	0.156	1.644
1,880	3.136	0.198	1.842
2,780	3.798	0.662	2.504
2,940	4.274	0.476	2.98
3,000	4.349	0.075	3.055
3,295	5.097	0.748	3.803
Bar left under strain at night and found broken in the morning.			

No. 504 was a bar of tin containing about 0.6 per cent. of copper—the opposite end of the scale—and exhibited precisely similar behavior, taking a set of

0.323 inch under 110 pounds and steadily giving way and deflecting uninterruptedly until the trial ended at the end of 1,270 minutes, over twenty-one hours. This bar, subsequently, was, by a maximum stress of 130 pounds, rapidly broken down to a deflection of 8.11 inches.

No. 501 presents the finest illustration yet entered in the record book of the Mechanical Laboratory of the Stevens Institute of Technology. The test extended over nearly $2\frac{1}{2}$ days under observation, and then left for the night, was found next morning broken. The time of fracture is therefore unknown, as is the ultimate deflection. The record is, however, sufficient to determine the law, and the strain-diagram (Plate II) is seen to be similar to that of the second test of No. 479, exhibiting the same tendency to the parabolic shape and the same change of law and reversal of curvature preceding final rupture, and illustrates even more strikingly the fact that this class of metals is not safe against final rupture, even though the load may have been borne a considerable time, and have apparently been shown, *by actual test*, to be capable of sustaining it. A strain-diagram of each of the latter two bars is exhibited on a reduced scale, to present to the eye, more strikingly, this important characteristic. (Plate II).

A comparison of the records and the strain-diagrams (Plate II), with those of Section I, in illustration of the behavior of the two classes of metals under constant deflection, is most instructive. The light thus thrown upon the phenomena of distortion and fracture may be of great service to all who are engaged in construction. It will be necessary to make many experiments to determine under what fraction of their ultimate resistance to rapidly applied and removed loads the members of the "tin-class"—the viscous metals—will be safe under static permanent loads. Their behavior under shocks of various intensities remains also to be determined. The most probable and most satisfactory conclusion which seems likely to be finally reached is, perhaps, that the "iron-class" of metals are capable of carrying indefinitely any load which they have once borne, and that, in some manner—by the relief of internal strain as suggested by the writer* or by some other process—their rest under a load renders them, as time goes on, more and more safe under that load.

The law of deflection and of rate of set, as illustrated graphically by the strain-diagrams given in this and in the preceding paper, is expressed for the lighter loads by equation of the form

$$Y = AT - BT^n$$

in which Y is the deflection or the set, both quantities varying together in this case, and T is the time; A and B being constant co-efficients to be determined for special cases.

For heavy loads, after the first sudden deflection and set, the equation is seen to be

$$Y = AT$$

in which for iron, $A = \frac{1}{Y}$ and for the tin-class A is a constant multiplier up to a limit x, Nos. 501, 479, at which it varies as some new function of the time.

The values of constants for the various metals remain to be determined. The question whether this change in the value of the Modulus of Rupture, as exhibited in the preceding section, and of the value of the quantity representing in the usual formulas the amount of deflection is due to a change in the Modulus of Elasticity, to simple flow, or to a variation of cohesive force, remains to be considered.

Section III.—Elevation of Elastic Limit in Gun Bronze.—The writer would refer to the recent criticisms of Prof. Kick, of Prague, on "Autographically produced Strain-Diagrams, and the Elevation of the elastic Limit by Strain." In a late issue of *Dingler's Polytechnisches Journal*, the Professor, in rejoinder to my reply to his criticism of my paper on the subject of the strength of materials and the elevation of the elastic limit by strain, asserts:

1°. That I use his formula, for determining the errors of apparatus due to velocity of motion, incorrectly.

2°. That I claim to be able to deduce the amount of work done in deformation of the test-piece from automatically produced diagrams in which the abscissas

* Wire makers have learned that newly made wire is considerably weaker than similar wire which has been so long made as to afford time for relief, by flow of the internal straining introduced by the process of drawing.

are proportional to the angular motion of the handle.

3°. That the error introduced by a blow will be greater as velocities are greater.

4°. That experimental proof of the elevation of the elastic limit in gun-bronze by strain was presented to Prof. Kick and others in August, 1873, by Gen. Uchatius, and that his discovery antedates that announced by me to this Society in November of that year.

I have been prevented by illness from noticing that statement before. I would now say:

1°. That I used his formula purposely as its author applied it, in criticising my paper, in order to make more striking the refutation. My reply is just as complete as if I had applied it to the more intricate case.

2°. That the abscissas of the strain-diagrams produced automatically are not proportional to the "motion of the handle," but to the distortion of the test-piece, and that this singular misapprehension of the subject of the criticism may be the result of that which prompted the original criticism.

3°. That I have distinctly disclaimed all intention of ascribing to the Autographic Recording Testing Machine the power of giving *quantitative* results when affected by shocks, and that my paper stands perfectly good, notwithstanding this fact, which was there implicitly stated, and would have been explicitly stated had it not seemed so perfectly obvious.

4°. That I shall endeavor, health and time permitting, to present proof that the experiments of Gen. Uchatius, in 1873 and before the date of my paper, do not prove an elevation of the elastic limit by strain; and, finally,

5°. That *gun-bronze does not possess this property.*

The phenomenon there shown was due, I think, simply to that condensation of metal by pressure, such as occurs in Whitworth steel and metals in which compression had similarly closed up the pores, and, by thus increasing their density, increased the resisting power of the metal.

The phenomenon which I have described in earlier papers as exhibited by autographic strain-diagrams and otherwise, the increase of the resistance of the material to distortion, is an effect, apparently, of internal molecular changes which do not affect density, and which, in tin and some other metals, result in a *depression of the elastic limit.* If my critic, or any other experimentalist, will study this phenomenon as I have done, he will find, I think, that gun-bronze belongs to what I have called the "tin-class," in which strain produces a *depression* of the elastic limit whenever any effect can be observed at all. I therefore think, the claim of my critic in behalf of the distinguished officer mentioned, can not be sustained.

THE DIFFERENT METHODS OF STEEL MANUFACTURE.

From "Iron."

THE manufacture of steel by a process as simple as possible, at the lowest cost and of the best quality, has called forth, especially of late years, the exercise of much inventive ability on the part of both chemists and engineers, both at home and abroad. There has resulted such a variety of differing methods that some systematic classification of the processes has become very necessary. In the *Mittheilungen des Hannoverschen Gewerbe-Vereines,* Professor Heeren publishes the complete classification, a translation of which is given below, and which will be found both instructive and of value for purposes of reference. Steel occupies nearly the middle place between cast and wrought iron in its proportion of carbon; it may be prepared either by decarburising pig-iron, or, on the contrary, by causing wrought iron to absorb carbon. The processes to accomplish these ends may be arranged under five principal heads: A, fabrication of steel by decarburization of crude or pig-iron; B, by carburization of wrought iron; C,

by mixing a wrought iron poor in carbon with pig-iron rich in the same; D, by by mixing pig-iron with ore (the pig yields carbon which reduces the ore and transforms the reduced iron into steel); E, directly by means of ore; F, cast steel. Subdividing these systems, we have the following methods under each heading:

A.—METHODS BY DECARBURISING THE CRUDE IRON.

(1) Steel obtained by a long heating of the crude iron in an oxidising atmosphere, the metal not being brought to fusion. (*a*) Turner's method in sand, where the deoxidation is produced by means of the oxygen in the air. (*b*) Jullien's method, in forged scales or spathic ore. This produces malleable iron. (*c*) Herzeele's method in steam. (*d*) Thomas' method in carbonic acid. The last two processes have not been employed to any great extent.

(2) Natural steel: In this method, employed since the earliest times, the crude iron is melted in a refining furnace with wood charcoal, and decarburised by the ferrous oxide of the scoria. The product is purified by a repeated refining.

(3) Puddling: This process is the same as the preceding, from a chemical point of view, but is practised in a reverberatory furnace heated with coal. It is necessary to purify the product by repeated refining, or by transforming it into cast steel.

The construction of puddling furnaces has undergone many changes. We distinguish (*a*) the ordinary puddling furnace with fixed hearth and heated by coal, (*b*) the same heated by lignite or peat, (*c*) the puddling furnaces of Schafthautl and others, with mechanical rabbles, designed to diminish the labor so fatiguing to the workman. These, however, have been entirely suspended by the new systems. (*d*) The Danks furnace, the hearth of which is formed of a hollow cylinder placed horizontally, and turning about its axis. It gives a product of exellent quality and is economical. The interior lining, however, is difficult to maintain. (*e*) The Ehrenworth furnace has a horizontal circular hearth, turning about a vertical axis. (*f*) The Pernot furnace also has a circular sole, which, however, is not horizontal, but slightly inclined, so that during its rotation the iron and scoriæ run to the lowest point, and are thus in a state of continual motion; while the elevated part of the hearth, together with the iron and scoriæ thereto adherent, are submitted to the oxidising action of the air. Professor Heeren thinks this furnace to be best, because it realises the advantages of mechanical puddling without needing any special lining.

(4) The Bessemer process: A current of air finely divided is passed through the liquid crude iron. The carbon, silicon and a part of the iron burn, and the temperature is so highly elevated that the iron, decarburised in part or transformed into steel, remains molten. It is then run into moulds.

(5) Berard's modification of the above: Air and gases are alternately introduced into the retort with different advantages.

(6) Peter's process: The liquefied crude iron in a reverberatory furnace falls in the form of rain, in a vertical chamber in which the furnace gases also pass, and in which air is blown so as to decarbonise the metal to the highest degree.

B.—METHODS BY CARBURISATION OF WROUGHT IRON.

(1) Indian or Wootz steel: Wrought iron of extraordinary purity, obtained by treating a very pure ore in small chamber furnaces by the direct method, is hammered, made into bars, cut into short pieces and placed in small crucibles with a few green leaves. The crucibles are hermetically sealed and heated for a long time at a high temperature. The iron is transformed into steel by uniting with it the carbon contained in the leaves and the steel even partially melts. These half melted masses furnish the famous sword blades and plates of Persia and Damascus.

(2) There are several other processes resembling the Indian, which, however, are not carried on on a large scale. There are (*a*) The Mushet process, in which wrought iron, obtained by the ordinary refining method, is melted with powdered wood charcoal. (*b*) The Vickers' process, analogous to the preceding, with the addition of oxide of manganese. (*c*) The Stourbridge, Brooman, Thomas and

Binks processes, based on identical principles.

(3) English cemented steel: Wrought iron of the best possible quality is, in the shape of bars, packed together in clay boxes, together with wood charcoal coarsely pulverised. The heating continues for two or three weeks. Without melting the iron is changed into steel, which by remelting is transformed into cast steel.

(4) Parry's cupola steel: Fragments of wrought iron, melted in the cupola with a large consumption of coke or wood charcoal, may be transformed into steel or even into cast iron, according to the length of the operation. This system offers an advantageous method of utilizing the scrap, and requires no special apparatus.

(5) Chenot's process: In this the ore is reduced by heating it progressively with coal. A non-melted iron sponge is obtained, which is ground and separated as far as possible from the gangues by the aid of a magnet. Lastly, it is mixed by carboniferous substances, and melted under pressure. The principal disadvantage of this process is the difficulty of separating the gangues without losing the steel.

6. Case-hardening has for its object the transformation of the surface of wrought-iron objects into steel. It is done in two ways. (*a*) The pieces are placed in small sheet iron boxes and surrounded with chips of wood. The boxes are hermetically closed and heated in a forge fire for fifteen or thirty minutes, to an intense red heat. They are then removed quickly, opened, and their contents thrown into cold water, thereby the exterior steel shell is rendered as hard as glass. (*b*) The pieces are heated to a whitish red and moistened with ferrocyanide of potassium, which acts by its cyanogen on the iron, and transforms the surface into steel.

C.—METHODS BY FUSION OF A MIXTURE OF CAST AND WROUGHT IRON.

The two materials may be both, or only one of them, used in a melted state.

(1) Bessemer steel, prepared by the ordinary method: The crude and wrought iron here are both liquid, while, as we have previously said, cast iron may be directly transformed into steel. The method most followed, and which leads most surely to the end in view, consists in completely decarburising the crude iron in the converter, and in adding to the melted metallic iron a rigorously-determined quality of liquid crude iron. The carbon of the latter affects the previously decarburised iron, and makes a steel containing a given proportion of carbon.

(2) Crucible steel is obtained by melting in crucibles a mixture of crude and wrought iron. The former liquefies first and slowly melts the latter.

(3) Martin's steel is similarly made by replacing the crucible with a reverberatory furnace. The crude iron is liquefied under a thin layer of scoria on the concave hearth of a reverberatory furnace, heated to an intense red-white heat by a Siemens regenerator. Scraps of steel and wrought iron of all kinds in desired quantity are added, and the steel is run into moulds of cast iron.

D.—METHODS BY A MIXTURE OF CAST IRON AND ORE.

Uchatius steel: The cast iron is granulated by running it into water while molten, and the grains are melted with spathic ore, peroxide of manganese and wrought iron in crucibles. The ferrous oxide of the spathic ore is reduced by the carbon of the cast iron, and the surplus of carbon unites with the wrought iron to make steel.

E.—METHODS BY PREPARATION DIRECT FROM THE ORE.

The Siemens direct process: The ore is melted alone, without addition of reducing material, at a very elevated temperature; then the iron is reduced and transformed into wrought iron or into steel by adding coal.

F.—CAST STEEL.

For the purification of steel by fusion, cemented, forged and puddled steel are employed. To improve the qualities of the steel, and notably to augment its hardness, diverse substances are added. Thus we have: (1) silver steel; (2) nickel steel, and (3) wolfram or Mushet special steel.

MOMENTUM AND VIS VIVA.

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III.

IV. THE PRESSURE PRODUCED BY IMPACT.

The best writers on the subject of impact recognize clearly that it is impossible to calculate, even approximately, the pressure which the accumulated energy of a moving body will enable it to produce by impact on another body, without we know beforehand just how much the two bodies will be compressed or forced out of shape by the expenditure of that energy, and also what is the law according to which the pressure varies as the distortion proceeds. This last requisite also implies, what must be evident by a little thought, that the total pressure between one body and another is not constant, but variable during even the short time of the impact. In "The Mechanics of Engineering," published by W. Whewell in 1841, there is a chapter on impact, in which he discusses the amount of compression and the pressure produced by the fall of an iron hammer from a certain height on an iron anvil; he also gives formulas for finding how far a given hammer will drive a nail or a pile, taking care to consider the effect of compression, as well as weight and friction. Many later writers have given rules for determining the pressure produced by impact, neglecting altogether the consideration of the amount of yielding or distortion of the substances, and thus making their rules of absolutely no value for general application. And we find, as we might expect, that these rules conflict with one another most remarkably.

The right view of the question is of practical importance, as the supposed results are sometimes made the basis for rules of safety in driving piles for foundations. We will, therefore, examine some of the principles concerning the pressure of impact which are laid down in text-books.

One author, who is in general very clear, states correctly the meaning of *momentum*, as already explained, but unfortunately goes on to say: "The momentum is also frequently called the *moving force* of the body, because it not only represents the intensity of the force required to overcome its motion, but also because the body would itself exert a force of this intensity against any obstacle tending to resist its motion." This statement would, therefore, make the pressure produced by a moving body when it strikes "any obstacle" equal simply to MV, or if we denote the pressure by P, and the weight of a body by W, previously denoted by G, we should have $P=\frac{WV}{32\frac{1}{6}}$.

Another common text-book states that "Beaufoy determined that a body of one lb. weight, with a velocity of one foot in a second strikes with a pressure equal to 0.5003 lbs. To find the pressure produced by the impact of any projectile, we have the general formula, Pressure$=0.5003\ MV^2$." The context shows that M in this equation stands simply for the *weight* of the moving body, and the formula will then be merely, $P=0.5003\ WV^2$.

Haswell, in his "Engineers and Mechanics' Pocket Book," p. 419, admits that in the case of a pile-driver the yielding of the pile will materially affect the "impact," but he then makes the following statement: "By my experiments in 1852, to determine the *dynamical* effect of a falling body, it appeared that while the effect was directly as the velocity, it was far greater than that estimated by the usual formula $\sqrt{s2g}$, which, for a weight of one lb. falling two feet, would be 11.34, giving a momentum of 11.34 ft.-lbs. (*sic*); whereas, by the effect shown by the record of actual observations, it would be $VW\ 4.426=50$ lbs." This result being given in *lbs.*, and not *foot-lbs.*, I suppose it is intended to represent the maximum pressure of the blow. Of course it would be absurd to suppose that a one lb. weight by falling two feet could acquire energy enough to perform fifty foot lbs. of work. Haswell's formu-

la I should then understand to be, $P = 4.426\ WV$*.

It is hardly worth while to add to the foregoing formulas one given by some writers, who are so careless as never to make a distinction between the *weight* and the *mass* of a body, and who also make the "moving force" equal simply to WV.

If we calculate by the preceding formulas the striking pressure of a 1,000 lb. cannon-ball having a velocity of 1,100 feet per second we have the following results:

$$P = \frac{WV}{32\frac{1}{6}} = 17.1 \text{ Tons.}$$

$$P = 0.5003\ WV^2 = 302{,}681.5 \text{ Tons.}$$

$$P = 4.426\ WV = 2{,}434.3 \text{ Tons.}$$

$$P = WV = 550.0 \text{ Tons.}$$

Where different formulas give results so astonishingly different as these it is not strange that a writer like Trautwine should say: "The force in lbs. with which a pile hammer makes its blow upon the head of a pile, cannot be calculated. All rules for that purpose are founded in error. We must here depend upon experience alone." Civ. Eng. Pocket Book, p. 321. We agree with this writer that in practical pile-driving it is impossible to calculate the pressure actually produced by the hammer on the pile. But the impossibility does not come from any real difficulty in the mathematics or the theory of impact, but simply from the fact that in practical cases we cannot get accurate data to work with. The intensity of the friction which resists the motion of the pile, changes with every blow of the ram, and so, probably, does the amount of compression and motion of the pile. Not knowing the circumstances of these, any calculations involving them will be liable to great error.

But it will be a help towards a true knowledge of the subject if we can have a formula which under certain supposable conditions will give us the maximum pressure due to impact, even though the conditions may not often be exactly fulfilled in practical cases. If we examine the foregoing formulas, we notice that that of Beaufoy makes the pressure vary as the *square* of the velocity, while all the others make it simply as the *velocity*. Haswell, *loc. cit.*, states that "the *effect* of the blow of a ram, or monkey, of a pile driver, is as the *square* of its velocity," but afterwards goes on, as before quoted, to give a formula from the record of actual observations, in which he makes the result in *pounds* vary as the *velocity* simply. This essential difference between Beaufoy and Haswell is of more importance than the difference of the coefficients in their formulas. But neither of those formulas involves the amount of yielding of the bodies between which impact occurs, and this defect alone would make any formula useless for general application.

To derive a formula for the pressure due to impact, involving the amount of yielding of the bodies, we must assume some law according to which the pressure shall be supposed to vary as the distortion proceeds. If an ordinary coiled spring is either stretched or compressed, its tension varies, within certain limits, directly in proportion to the amount of distortion from the position of rest, the tension being zero in this position. Suppose a spring following this law to be itself without weight, and let an incompressible body whose weight is W be projected against the spring with the velocity V, producing a movement of the free end of the spring through a distance d in stopping the projected body, what will be the number of pounds pressure on the spring at the instant of its greatest compression?

We may easily compute this pressure in terms of the quantity of *work* required to compress the spring through the given distance. For the *work* required is equal to the *mean* pressure multiplied by the distance, and if P represent the pressure at the instant of greatest compression, the mean pressure will, by the law of the spring, be $\frac{P}{2}$. If we denote the necessary quantity of work by Q, we shall then have $Q = \frac{P}{2} d$,

whence, $$P = \frac{2Q}{d} \quad . \ . \ . \quad (1)$$

Now we have seen that the work

* Since completing this article I have seen a curious discussion in the July and August Nos. of *The Journal of the Franklin Institute*, between Nystrom and Haswell, but without access to the details of Haswell's experiments, I do not see as it requires any change in my presentation of the subject.

which a moving body can perform in virtue of its velocity is $\frac{1}{2}MV^2$, or $\frac{WV^2}{64\frac{1}{3}}$. Putting this in the place of Q, in equation (1), we have for the pressure at the instant of greatest compresssion,

$$P=\frac{WV^2}{32\frac{1}{6}d} \quad . \quad . \quad . \quad . \quad (2)$$

The constant, $32\frac{1}{6}$, in this formula, supposes the value of V to be expressed in feet per second. In applying the formula, therefore, d must be reduced to feet or fractions of a foot. W may be expressed in any unit of weight, and P will then be in the same.

We have supposed the moving body to be incompressible, and the spring to yield with a resistance increasing uniformly with the amount of its compression. In actual cases of impact between two solids, their molecular springs would take the place of the coiled spring, and both bodies would yield more or less; but if we suppose the resistance to increase according to the same law, a similar formula would apply, assuming d as the distance passed through by the center of gravity of the moving body during impact. Now it is a fact shown by experiment, that within certain small limits any displacement of the molecules of an elastic solid, develops a resistance increasing directly as the amount of the displacement. Hence, within these limits, (which the form of the impinging bodies would modify) in a case where d could be determined, and the modulus of elasticity were known, we could calculate with some accuracy the maximum pressure produced in the impact of a moving solid upon a fixed obstacle. If the law of the increase of the resistance is not known, the pressure cannot be calculated.

From the formula, $P=\frac{WV^2}{32\frac{1}{6}d}$, it appears that, other things being equal, the pressure in case of impact is inversely proportional to the distance in which the moving body is brought to rest. The harder and more incompressible the substances, then, other things being equal, the greater will be the pressure produced by impact. If both the substances between which impact occurs were perfectly incompressible, the pressure from impact would be infinitely great, however small the mass or velocity of the moving body. It would therefore evidently be impossible, with any resistance, however great, to stop in *no* distance an incompressible body, however small, or moving however slowly.

It also appears from the formula, that the pressure from impact increases, *other things being equal*, with the *square* of the velocity of the moving body, instead of simply with the first power of it. How experimenters might be led to the opposite conclusion will be suggested further on.

In the Spring of 1870 I performed a series of experiments for comparison with the formula now given,

$$P=\frac{WV^2}{32\frac{1}{6}d}.$$

I found that I could measure the pressure of impact by the expansion of a spring, with less liability to error than by its compression. After a few preliminary trials I adopted the following plan:

A coiled steel wire spring about a foot in length and weighing three ounces was firmly suspended by one end. Screwed into the bottom of the spring was a small brass thimble, pierced in its center to allow the free passage of a steel wire up through the spring. To this wire near the middle was fitted tightly a small brass collar which could not pass below the thimble, and to the lower end of the wire a cylindrical iron weight was attached. An arrangement for supporting and dropping the weight accurately was made by filing a small notch in the upper part of the wire, which projected up through a hole in the support of the spring. After the weight had fallen freely a certain distance, the collar on the wire would catch on the thimble at the bottom of the spring, and extend the spring till its resistance stopped the weight and caused it to fly up again.

Behind the spring and weight thus arranged, a scale of equal parts was placed perpendicularly, so that the height at which the weight was supported could be accurately measured by placing a small square against the scale, under the weight. To measure to what distance the weight fell when dropped so as to stretch the spring, I first determined the lowest point approximately, and then arranged just beneath it a small wooden

needle stuck into a dish of light meal; then raising this needle so that when the weight fell it would drive the needle down a very little and leave it at the lowest point, the extent of the fall could be very accurately determined.

Knowing then, the entire fall of the weight, and the extent to which it stretched the spring, and assuming that the resistance of the spring increased in proportion to the extension, (which is assumed in the ordinary spring balance, and which I found by frequent weighings to be quite closely true), and neglecting a slight loss from friction and heat, I had the data for calculating the maximum pressure which the impact of the weight should produce. (I have also neglected the weight of the spring, assuming that whatever work is spent in giving it vis viva will be given out again by it, by the time the greatest compression is reached). By suspending to the weight an extra load sufficient to draw it down till the extension of the spring was the same as that caused by impact, and weighing the whole load, the actual maximum pressure produced by impact was found, and compared with that calculated by the formula.

In calculating the pressure from the formula, the value of V is taken at what it would be if the weight fell freely for the whole distance, including the extension of the spring. This value is, of course, somewhat greater than the velocity actually attained by the weight; but there is a compensation in the action of gravity on the weight during the extension of the spring. This method reduces the calculation to what it would be if the weight were moving horizontally with the assumed velocity, and extended the spring by its vis viva only, without the assistance of gravity during the extension.

In the first of these experiments the weight of the drop was 14.79 ounces. A mean of ten trials, with an extreme variation from the mean of $\frac{1}{10}$th of an inch, gave for the whole fall of the weight 21.26 inches, and for the extension of the spring 10.82 inches. From these data the formula $P=\frac{WV^2}{32\frac{1}{6}d}$ would give a pressure equal to 58.1 ounces. The total observed steady weight required to extend the spring to the same degree was found to be 55.6 ounces, falling short of the calculated pressure by 2.5 ounces, or about $\frac{1}{23}$rd of the calculated pressure. Two other sets of observations under similar conditions, one of which is the first in the table below, gave results not differing materially from this.

In order to further extend my experiments, I suspended the same spring by a support near its middle, so as to have the effective length of the spring, when at rest, about five inches; making virtually a new and stiffer spring. I also procured an additional spring, about seventeen inches in length, of heavier wire and wound on a smaller core, so as to make a still stiffer spring. Its weight was about five ounces. It was fitted up like the former one, a little more care being taken in details. I experimented with this spring and a drop of about three pounds weight, and also with the drop used before, using first the full length of the spring, and afterwards cutting it in two, thus once more increasing the stiffness of the dynamometer.

I give a summary of the means of nine sets of experiments, separating them into divisions according as they were made with springs of different stiffness, increasing from I to IV:

(*See Table on following page.*)

It was to be expected that the tension calculated by the formula $P=\frac{WV^2}{32\frac{1}{6}d}$ would be a little in excess of the observed tension; for some of the *work* done would be wasted on friction, atmospheric resistance, &c. And in fact, all of the observed tensions are found to fall a little short of those calculated by this formula, yet approximating so closely to them, under a variety of conditions, that the differences may fairly be ascribed to these unconsidered losses. I found in general that with an increase of velocity, other things being the same, the error slightly increased, the proportional effect of these resistances being greater. Also the proportional error was greater when the suddenness of the shock was increased, as in experiments 6 and 7, which were made with the small drop and the stiffest spring.

The co-efficients in the formulas $P = 4.426\,WV$ and $P=0.5003\ WV^2$ are such that with values of V below 8.85

Stiffness of spring.	Reference No.	Weight of drop in ounces.	Height of fall in inches.	Velocity due to height in feet per second.	Extension of spring in inches.	Tension in oz. by $P=4.426\,WV$.	Tension in oz. by $P=0.5003\,WV$	Tension in oz. by $P=\frac{WV^2}{32\frac{1}{6}d}$.	Tension in oz. by observation	Ratio of excess by formula $P=\frac{WV^2}{32\frac{1}{6}d}$.
I	1	14.79	21.28	10.67	10.84	698.4	842.6	58.07	55.67	$\frac{1}{24}$
II	2	14.79	9.13	6.99	4.57	457.6	361.5	59.07	57.88	$\frac{1}{49}$
III	3	49.414	10.02	7.33	7.12	1,603.1	1,328.2	139.08	136.03	$\frac{1}{46}$
	4	49.414	14.35	8.77	8.55	1,918.0	1,901.2	165.87	162.96	$\frac{1}{57}$
	5	49.414	20.43	10.46	10.17	2,287.6	2,704.6	198.53	193.91	$\frac{1}{43}$
IV	6	14.79	4.51	4.92	1.89	322 0	179.1	70.58	66.66	$\frac{1}{18}$
	7	14.79	10.62	7.54	2.90	493.6	420.7	108.32	102.28	$\frac{1}{18}$
	8	49.414	8.51	6.75	4.85	1,476.2	1,126.2	173.41	170.91	$\frac{1}{69}$
	9	49.414	13 96	8.65	6.17	1,891.8	1,849.6	223.61	217.43	$\frac{1}{35}$

the pressure calculated by the former will be greater than that by the latter, but with values of V above 8.85 the reverse will be the case. It happens that in case of my experiments both of those formulas give results very much in excess of the observed pressures. This would not always be so. If I had used a very stiff dynamometer some of the tensions calculated by those formulas might have fallen below the observed, instead of above. I purposely employed springs that should yield considerably, so that the distance the drop passed while coming to rest might be more readily and accurately measured.

A discussion of the results of the table will show how experiments with regard to impact might be conducted, which should apparently lead to the conclusion that the resulting pressure is proportional simply to the first power of the velocity. A comparison of experiments 3, 4 and 5, which were performed with a single spring and a single drop, but at different velocities of this, shows that the observed pressures actually produced in these experiments, were very closely proportional to the velocities. If there were only those three experiments it might be assumed from them that the maximum pressure in case of impact would be equal to the product of the weight of a moving body and its velocity, multiplied by a certain constant. Making this assumption, I should find from the *observed* pressure in No. 3, the value of the constant to be 0.3755; from No. 4 the constant would be 0.3761; from No. 5 it would be 0.3751. The mean of these three nearly accordant values would be 0.3756. Now the formula $P=0.3756\,WV$ would apply very closely to any number of experiments, made with the same spring and the same moving weight at various velocities, and would apparently show that the pressure in case of impact varies as the first power of the velocity of the moving mass.

But we shall find that this formula will not apply, with any approach to accuracy, to the impact of the same weight on a spring of different stiffness. Suppose we calculate the pressure which this formula would give when applied to experiments 8 and 9, which were made with the same moving weight, but with a different spring. From the given weight and velocity in these experiments, this formula, $P = 0.3756\,WV$ would make the pressure in No. 8 equal to 125.28, against 170.91 observed, and in No. 9 equal to 160.54, against 217.43 observed, results which show the formula to be useless for the spring used in 8 and 9. But the two observed pressures in 8 and 9 are very closely proportional to the velocities given in them; hence we can easily make a formula of the same sort, which would apply for this spring and weight. Thus if we suppose the pressure to be equal to the product of weight and velocity by a constant, the observations of No. 8 would make the value of the constant 0.512, and those of No. 9 would make it 0.509, the mean being, say, 0.511. Hence the formula, $P = 0.511\,WV$ would apply pretty closely to an unlimited number of experiments, made at various velocities, with the same spring and falling weight as those used in experiments 8 and 9.

But if a similar formula were made from the observations of experiment 1, we should get still another constant for that spring and weight, viz: 0.353; and from the observations of No. 2 yet another constant for that spring and weight, viz: 0.560; and lastly, for the spring and weight of Nos. 6 and 7 we should need a different constant from all these, viz: 0.917. Thus it appears that the supposed constant for such a formula is no constant, but varies with every change in the dynamometer used to stop the weight.

However, we have now seen that *with a given dynamometer* and *a given moving weight*, the maximum pressure of impact varies simply as the first power of the velocity. The reason for this is easily shown, if we consider the *work* required to stretch a given spring to a given tension. We have seen that this work is represented by $Q=\frac{Pd}{2}$. But, by the law of the spring, P is proportional to d, and if C represent a constant, we may write $P=Cd$, or $d=\frac{P}{C}$, which value for d, when substituted in the value of Q just given, produces $Q=\frac{P^2}{2C}$; that is, the quantity of work required to stretch the spring to a given tension P, varies as the *square* of that tension. In order, therefore, to produce a *double* pressure on *a given dynamometer* by a given moving body, we must *quadruple* its power of performing work; which we know, from the principle of vis viva, will be done by simply *doubling* its velocity. Or in general we must make the velocity proportional to the pressure to be produced.

But if we were to double the velocity of the moving body, and let it be stopped by a different dynamometer, stiff enough to stop it in the *same distance* as before, we should of course find a quadruple pressure. If the distance d remains the same, the pressure must vary as the *square* of the velocity. If this distance is not taken account of, it would, however, be no more correct to say that the pressure varies as the square of the velocity, than as the velocity simply; for the pressures calculated by the formula $P=0.5003\,WV^2$ are not only widely at variance with the results of my experiments, but no two of the pressures calculated by that formula have a common ratio to the observed pressures. But the formula $P=\frac{WV^2}{32\frac{1}{6}d}$ applies with reasonable accuracy to all the experiments of the foregoing table.

We have next to inquire in what ratio the pressure changes if the dynamometer remains the same and the moving weight is changed. We are usually taught that the pressure of impact varies directly as the weight of the moving body. And the formula $P=\frac{WV^2}{32\frac{1}{6}d}$ shows that if V and d remain unchanged, P will vary in the same ratio as W varies. But if we do not change the dynamometer or the velocity of the moving body, an increase of W will necessarily increase d, hence P will not in this case vary in the same ratio as W. And we have seen that experiments 8 and 9 may be represented by the formula $P=0.511\,WV$; whereas Nos. 6 and 7, which were made with the same dynamometer but with a different moving weight, would need a different constant, requiring the formula to be $P=0.917\,WV$. If we were to apply the formula $P=0.511\,WV$ to No. 6, we should get $P=37.11$, against 66.66 observed; and the same formula applied to No. 7 would give $P=56.87$, against 102.28 observed; results very wide of the mark. What then is the true ratio of change of pressure, keeping a single dynamometer, but changing the weight of the moving body?

To answer this, suppose that a certain weight with a certain velocity produces a certain pressure. To produce a *double* pressure on the same spring by another moving weight having the same velocity as the first, we know from what precedes that we must perform *four* times the work; hence this moving weight must be *four* times as heavy as the first. And in general, the velocity at impact being constant, the maximum pressure on *a given dynamometer* will only vary as the *square root* of the moving weight, instead of as the weight.

It is wrong then to suppose that even with a single dynamometer, the pressures produced by different moving bodies

may be found by multiplying the product of the weight and velocity by a constant. We may, however, as is now evident, properly assume that *with a given dynamometer* the pressure of impact will equal some constant multiplied by the product of the velocity and the *square root* of the weight. Making this assumption the observed pressure in the 8th experiment would give 3.602 as the value of the constant for that dynamometer, and No. 9 would give 3.576, the mean of the two being 3.589. We may now write a formula, $P=3.589\,V\sqrt{W}$, which will apply to various weights and various velocities, with the given dynamometer. Applying this new formula to experiments 6 and 7, made with the other weight, we find for No. 6,

$P=3.589\,V\sqrt{W}=67.9$, and for No. 7,

$P=104.05$;

both results being very nearly the same as the observed, the slight excess being due to the fact of greater proportional unconsidered resistances in these two experiments than in 8 and 9.

We have thus shown that for a *given dynamometer*, yielding by the assumed law, we may write an equation of the form $P=CV\sqrt{W}$, which shall leave out of view the amount of yielding, and yet give the maximum pressure due to the impact of various weights at various velocities. But the value of C will change with every change in the stiffness of the dynamometer, and hence the formula is not of general application.

This discussion leads me to infer that the experiments from which Haswell deduced his formula must all have been made with a single dynamometer and a single weight; otherwise it would hardly seem possible that he could have obtained such a formula as he gives, with a single and invariable coefficient. A striking illustration of the increase of pressure by increasing the stiffness of the dynamometer is seen by comparing experiments 1 and 6 of my table. In 1, a fall of 21.28 inches gave an observed pressure of 55 ounces, while in 6, with a stiffer spring, the same weight, falling only 4.51 inches produced an observed pressure of 66 ounces.

I will add a brief account of some experiments made in connection with the foregoing, but under a different condition of the dynamometer. In the preceding experiments, the resistance met by the moving body at the instant of first striking the dynamometer was zero, and increased uniformly to the maximum. I wished to try the experiment of having the dynamometer already under a tension on being struck by the weight. I accordingly arranged some pins so as to hold the spring stretched to a certain tension, and then allowed the weight to fall and continue the extension of the spring until the blow was spent. For calculating the maximum pressure which should be produced by the fall of the weight under these conditions, a modification of the formula becomes necessary.

If P' represent the initial tension at which the spring is held, and P the tension after being extended through d additional units of distance, and Q the *work* necessary to produce that extension, the work will be equal to the *mean* tension multiplied by the distance, giving

$$Q=\frac{P+P'}{2}d\,;$$

whence

$$P=\frac{2Q}{d}-P'$$

Substituting for Q its value, $\frac{WV^2}{64\frac{1}{3}}$ we get

$$P=\frac{WV^2}{32\frac{1}{6}d}-P' \quad . \quad . \quad (3)$$

The spring used in experiment 1 was taken, and was drawn down a certain distance and held by the pins as explained, giving a tension of $P'=14.9$ ounces. A total fall of the weight, (mean of ten trials), was 11.71 inches. The extension of the spring produced by the fall was $d=5.69$ inches. The value of W was 14.79 ounces, as before. These values in the above equation give for the maximum pressure of the impact under the condition described, $P=45.4$ ounces. By trial with steady weights, extending the spring to the same distance, the observed tension was 43.8 ounces, falling short 1.6 ounces, or $\frac{1}{28}$th of the calculated tension. A repetition of the experiment under similar conditions gave me a calculated value of $P=45.3$ ounces, and an observed value $P=44$ ounces; the error being 1.3 ounces, or $\frac{1}{34}$th of the calculated value.

These experiments show that if we know the laws of change governing the yielding of substances during impact, we can calculate closely the maximum pressure produced; otherwise we cannot be sure of the least approach to accuracy. In the experiment just described, if the maximum pressure were calculated under the supposition that it would be the same as if the weight were brought to rest in the observed distance by a spring stiff enough to do it without any initial tension, the value of P would be 60.3 ounces, against 43.8 ounces observed, the excess being more than a third of the latter. But by the proper modification of the formula, a fairly close approximation to the observed result was calculated.

THE FUTURE OF SANITARY SCIENCE—POLITICAL, MEDICAL AND SOCIAL.*

From "Nature."

I COULD have wished it had been in my power on the present occasion to produce one of those essays which appeal to the imagination while they prepare the mind for the reception of sanitary principles and practice. Such essays are tempting and, in their place, instructive. To-day I am bound on a voyage less pleasant, yet I hope not less useful.

There has recently been called into existence a new society under whose summons we now meet. The society has assumed to itself the expressive name of the Sanitary Institute of Great Britain. It starts as a voluntary effort by men and women who are willing and anxious to give effect to those teachings of sanitary science which the past half-century has revealed. It invites all who are concerned to utilize the knowledge that has been acquired in that time. It wishes to encourage new research. But it has for its most anxious care to render useful to mankind at large the accumulated store of knowledge which at this moment lies ready for so many grand purposes relating to health. It accepts as its object, work for health, health of all the human family.

Shall some one say the object is ambitious? Yea, we reply, it is confessedly ambitious. Shall some one say the means at command for the work to be attempted are weak? Even so. Life is short, art long. Yet the short yields the long, and but for the short the long could not be. It is out of these littlenesses of human effort that the greatnesses follow. Or, as Benjamin Rush very forcibly puts it, and simply as forcibly: "There are mites in science as well as in charity, and the ultimate results of each are often alike important and beneficial."

It is my fortune, good or bad, to have to preside over the council of this new society. Of the ability of those who form the council, and of their experience, I need not speak in detail, for their names are familiar to the world. They represent, I may say, sanitary science in all its branches, and from them, working harmoniously together, good results must be expected.

It seems fitting therefore as we enter on our work to look forward to the future. It is a part at least of our duty to look towards the future with the view of seeing in what directions we may best proceed; what assistances we may have to call upon; and chiefly what great powers we may have to consult and propitiate.

The three great powers with which our society will have to treat are the political, the medical, the social. From each of these we shall expect constant assistance. To one or other of these, whatever we do, our work will be transmitted or transferred. They will bring it into practical form and effect, or they will reduce it to nothingness. We can suggest and set forth initiatives, and with that our functions are complete in each particular branch to which we address ourselves.

It is our special duty to keep this

* An address delivered before the Sanitary Institute of Great Britain at the Royal Institution, on July 5, 1877, by Benjamin W. Richardson, M.D., LL.D., F.R.S.

special fact steadily in view and to limit our labors by it. It too often happens that young societies like young men are apt to believe that they can conduct national processes as easily as they can conceive them, and under this belief fail most signally with the best of attempts. I remember in my early career getting a lesson from one of our late well-known statesmen on this very point. I was explaining to him the efforts I had made in 1855 and the succeeding three years to establish a registration of the diseases of this kingdom, and I bewailed the hard experience which proved that the greater the scientific success of the effort the more impossible it became to carry it out. In fact, said I, in a pitiful strain, the success almost ruined me in mind, body, and estate. "Served you right," was the immediate reply, "Served you right. If individual men could carry out national projects where would be the nation?" The reply was hard as it was unanswerable, and from that time to this I have given up all thoughts of doing more than sowing seed in the field of literature and leaving it to the chance of fructification on that extensive soil; or in showing some mere model of experiment which, perchance, may grow into working form. And this, I think, is the whole natural scope of our Institute,—to sow the seed of sanitation; to think our plans of projects for working methods; to lend its many minds, as if they made up the mind of one man, for devising from the past the best for the present, and respectfully to declare our conclusions.

The directions in which we shall have to move, the lines on which we shall have to move, are, I repeat, chiefly three —the political, the medical, the social. The powers on these lines must be approached in every work of ours, however simple, however complicated it may be. I shall try, as the title of my discourse explains, to indicate certain points in which we are most likely to come in contact with these powers and the changes we may expect to work in and through them.

THE POLITICAL PART.

In this country political action has been varied in relation to sanitary improvements. Sometimes political necessity has crossed sanitary progress, as, for example, in the imposition of a tax on sunlight, on foods that are essential to life, and in the granting of licences for the sale of pernicious drinks. At other times, and by fits and starts, political action has been in aid of sanitary work. So far back as the reign of Edward the Third, 1361, a royal proclamation was made through Parliament for preventing the slaughter of cattle in the streets of London because of the pollution of the streets and the drains which arose from that cause. From that time under great emergencies other similar acts came into force. They rarely lasted very long. As the urgent necessity for their existence passed away, they were allowed to fall into abeyance, and no permanent machinery was kept in order for insuring their continued and effective action.

Let me not, however, in saying this, be understood as conveying any special charge of neglect against English legislation. It is just to state, as an historical fact most creditable to our national history, that our legislators have by a long precedence taken the lead in sanitary affairs over those of other nations. In 1802 the great sanitary act for regulating the labor of children in factories set the example from which much useful legislation has followed at home and abroad. In 1838 that great original sanitary scheme for the registration of the births and deaths of the kingdom was inaugurated, to become a collection of facts relating to life, and disease, and death, of which there is elsewhere no parallel. And, since the era of the Crimean campaign, so much legislation has been attempted bearing on health, I dare not attempt even to enumerate the titles of the different measures that have been introduced. At this moment there can be no doubt as to the sincerity of our governments, of whatever party they may be composed, for dealing with every subject relating to the public health in an efficient manner, and in as rapid a progression as the slow and sure mode of parliamentary procedure will permit. The subject indeed presses at this moment with so much force on the governing mind, that if there be any danger ahead it is the danger of too miraculous a draught of small enactments, to the ex-

clusion of comprehensive measures which all who run may read.

In saying this it is necessary to guard myself against error of expression. By comparison with all the nations of the world beside, we have obtained legislative measures which are splendidly comprehensive. No other country in the world can present an approach to the Public Health Act of 1875. That Act, as far as it goes, is admirably constructed. Its constitution of sanitary authorities throughout the kingdom; the power it vests in those authorities to appoint learned medical officers of health; the provisions it makes for securing to each locality better sewerage, freedom from nuisances, improved water supply, regulation of cellar dwellings, governance over offensive trades, and removal of unsound foods; the provisions for prevention of spread of infection and for the erection of hospitals and mortuaries; and the provisions for the regulation of streets and highways, lighting of streets, establishment of pleasure grounds, and regulation of slaughter houses; these, as well as the general provisions for the carrying out of the Act, are most commendable as practical plans by the working of which the nation may be tempered into sanitary mould of thought and character.

In a word the Act of 1875 is an improvement of the first degree on all that has preceded it, and although much of it, by the necessities of the constitution of our country,—which recognises the domination of free will even in its age of ignorance,—of a permissive nature, the working of the Act must in a few years remove a great amount of disease from the land and prevent the invasion of diseases of an epidemic and spreading type.

Sanitation however admits of being studied from two distinct points of view, the legislative and the scientific. The legislator may say, and perhaps with justice, that the production of such a measure as the Act of which I now speak is as much as can be done. The man of science may say that this is childish talk, that much more requires to be done, and that after all that which has been done, though it be comparatively great, is practically imperfect and very little. Science in this respect is always in advance of legislation, and that is her true place,—the pioneer's place. I remember the time perfectly when every fragment of the Public Health Act of 1875 was in the hands of men of science solely, and was called a chimera, over which great lawgivers shook their wise heads and passed by.

At this moment the positions of science and legislation are relatively the same as they have ever been, and it is fair for us men of science now as in the past time to declare the way ahead for the law-maker. I shall proceed again, therefore, as I have often before, to indicate one or two new starts in sanitary legislation, not from the legislative but from the purely scientific point of view, uninfluenced by the many and vehement individual grievances and troubles which beset the path of the minister of state. In so doing I shall indicate also, by inference, what I think our society ought to support in the sanitary policy of the future.

In the first place, then, we ought to expect in the political progress of sanitation that there will be established in connection with the Government one central department in which every subject, directly and even indirectly, connected with the health of the people, will be considered. This department, it is to be hoped, will be under the control of a Cabinet Minister, and will supervise the sanitary work performed at present by the Local Government Board, the Registrar-General's department, the sanitary regulations of jails and reformatories, and all the duties now pertaining to the supervision of factories, in so far as the health of the employed is concerned: in fine, every sanitary work that can be weeded out of every other department of the state.

To such a central board or department a specific name is necessary. The name should be as distinct as that of the department for war, for the navy, for the exchequer, or for the post-office. The name, it is to be hoped, will be emphatically the Health Department, and the chief of it the Minister of Health.

It may be urged that substantially we are drifting into some such order as is here suggested. It may be urged that the Local Government Board is step by step assuming the duties assigned, as

above, for the State Department of Health. To some extent this is true, and it might be advisable, for the sake of the connection which must always exist between such a central board and the various local boards in the kingdom, to add to the name of Health Department that of Local Government Board. But for the sanitary object the leading name must be Health, and Local Government must come in merely as indicative of the connections that exist between the State and the local centres—as the machinery.

In this question of progress there is involved an immense deal in a name. It is essential to the scientific sanitary teacher that every reasoning mind in the kingdom should become familiar with the two significant words, public health, or national health. It is equally necessary to let the people know fully that the Government has the health of the country under its general and wise supervision. But it is utterly impossible to make either of these facts understood by the masses so long as any sanitary authority, central or local, has a title which fails to convey the meaning of its functions. To speak to the masses who are listening to a lecture or discourse on health about a local government board is only to confuse them. They ask you afterwards what it all means, and they go away imbued with the impression that it means anything except what relates to the health of the people.

I am speaking very practically in suggesting, that in the course of political sanitary progress it is an absolute necessity for success to give its proper and only name to the department of state which presides over the national health. I do not state too much in declaring that every public measure would carry more weight if it went forth as being under the supervision of the health department. It may appear a refinement of illustration, and yet it is a sound argument that vaccination would have met and would meet with far less opposition if it were enforced under the general supervision of a State department of health. As it is the people connect the carrying out of vaccination with something other than health, and even as distinct from the idea of conservation of health. It is looked upon as a legal tyranny, having no scientific setting forth of its intention, and as springing from no scientific authority. If you attempt to reason with its active opponents on the subject, and refer to the authority that exists, they dispute the competency of the authority in name and form; and, foolish as the objection may be, it is potent for obstruction.

In making this suggestion there is no necessity to offer a word against the continued action of local self-government. The work of the local centers in all parts of the kingdom instead of being in any degree curtailed and restrained, should be encouraged and maintained. In the sanitary local work the word health should, however, again come forward as the one prominent designating term to which all others should be subject.

Our Sanitary Institute could not turn its attention to any more suitable labor than that of inculcating the necessity for the institution of one state department exclusively devoted to the health of the people. In the success attending such an effort a double result would be achieved. The country would have secured for it the best and most direct guidance on its most vital interest, and scope would be given to the industry of men of science in a new direction. Men, whose lives have been devoted to the study of life and health, would be prepared by their devotion for the accepted service of their country in public form, and the Houses of Parliament would become, at last, congenial spheres for their labors. The Houses would be strengthened by such adhesions; the men would be more useful and honored.

Another work in the political line which will be demanded in the future for the benefit of the sanitary cause is the preparation of such a digest of all our practical sanitary laws that every person of intelligence can read and understand what may be legally enforced for the maintenance of health. What may be done in this direction ought to be so simple and so plain as to be brought into a school-book. Not a line should be left for the subtlety of the legal brain to twist into contortioned illegibility. The laws by which the health of a man, and thereby of a nation,

can be preserved to the utmost, are so simple in nature that nothing but the utmost simplicity can truly express them, and the whole labor of the future, if it is to be of any service whatever, must be directed to the discovery and establishment of such simplicity of exposition and direction. Up to the present time much that has been done has been provoked by that most untrustworthy of all human provocatives to action,—fear. Some great epidemic has occurred that has caused universal dismay; some great catastrophe has occurred, like that of the Crimean campaign, which has excited universal criticism on the failure of sanitary provisions by the authorities of the nation. Some such slip has been permitted in sanitary rule as that which recently let scurvy undermine the workers during a great enterprise of discovery. Straightway on the heels of such events there have been commissions of inquiry, and as a direct or indirect result there has often come forth some particular enactment. Or—and this is by no means rare—some individual of the House of Commons, impressed with the danger of a great national evil, has pressed for a national remedy, and, by steady persistance session after session, and by showing that he never knows when he is beaten, has forced the Government to take up his measure and to carry it through.

From these modes of legislating for health we have obtained many minor acts which fill and refill the national statute books. And still this process promises to go on, a process of labor in a circle with much loss of time and expenditure of force without ultimate progression.

It would be vain to find fault with the past for its doings. As vain to find fault with the State for meeting State disorders by empirical remedies as it would be to find fault with the physicians of a former day for the same mode of procedure. If the people demand a recipe they must have it, be it from the State or the family physician. The question that now comes forward is whether the time has not arrived for ceasing to treat the health of the nation by specific or supposed specific remedies for particular errors, and whether we may not find in the future a few very simple and natural guiding principles on which all acts of Parliament relating to the health of the people may be based?

Before this effort can be attempted the existing acts that touch on health,—public health acts, metropolitan health acts, contagious diseases acts, vaccination acts, factory acts, acts relating to the importation of cattle, adulteration acts, and others relating to prisons, work-houses, and the like, and which, if they even lie latent are not repealed,—these, one and all require to be considered together, with the view of determining whether an English or even a British act of settlement for the vital regeneration of the realm is not practicable on a simple natural basis of natural requirement.

I am fully aware that this suggestion carries with it the idea of a gigantic labor; but it will have to be done, and once fairly tackled I dare say the apparent difficulties will readily dissolve away. It is a mere question between doubting and attempting: and we all know and feel that—

> "Our doubts are traitors,
> And make us lose the good we oft might win
> By fearing to attempt."

Supposing the existence of an efficient central department of health acting under the direction of a minister of health, a grand new duty, as it seems to me, would be to determine what is the evil or what are the evils that have to be removed in order that the cleanest bills of health may be regularly presented to the nation. Without such preliminary knowledge all sanitary work is unsound to the last degree. It were as wise for me to write a prescription for a man without inquiring into his disease, his antecedents, and modes of life, as for the State physician to prescribe for national sickness without inquiry into the nature of the sickness, its antecedents, and the cause or causes that led up to it. The great work, therefore, and indeed the first sanitary work of the future, standing before all other sanitary legislations except the formation of the central authority, is the systematic enumeration, week by week, of the diseases of the kingdom, through the length and breadth of the kingdom. It is utterly hopeless to attempt any decisive measure for lessening the mortality,

which is certainly more than double what it ought to be, until this State labor is faithfully carried out. It is vain, comparatively speaking, to know what totality disease hands over to death, unless we know also what health under one or other cause of disturbance yields over to disease. Physicians and statists strain their eyes to try to get at the extent of disease. Laborious geographers like Mr. Haviland spend years in constructing maps from the tables of mortality, in order to get a mere approximation of the distribution of disease in England: and meanwhile disease itself, constantly cheating the observers, is making its way without being under any systematized recorded observation.

For the omission of a registration of disease there is no conceivable excuse. The thing has only to be done. The organization of the Registrar General's department has fully opened the way to the collection and the utilization of the facts relating to birth and death. These elements swing in the statist's balance readily, and are weighed by our consummate state weigher of life and death, Dr. Farr, as accurately as the Chancellor of the Exchequer balances the national ledger. With equal readiness Dr. Farr, if the data were collected for him, could tell from week to week the health as well as the mortality of the kingdom. In a short time, under such regular record, the whole nation would know the reigning health, the reigning disease, of every center of life. And if, as might easily be done, the diseases of the lower animals and the diseases of the vegetable kingdom were included in the returns, all the facts of disease would be completely rendered.

I think I have already referred to an effort I made many years ago to carry out this design of registering the diseases of the kingdom. I refer to that effort again for a simple reason,—for the purpose of indicating that there is really no greater difficulty in getting the facts than there is in utilizing them. I attempted no more than the registration of the epidemic diseases, and I could afford no more than the publication of a quarterly abstract of the data that were forwarded. But in a short time fifty medical observers were sending in returns from as many stations, extending from St. Mary's, Scilly, to Lerwick, in the Shetland Islands. These stations could easily have been increased to any extent, and the amount of information regularly communicated was indeed most valuable.

Two facts connected with this attempt are perhaps worthy of note, one as showing something determined, and the other as showing something suggested. In the returns sent from the district of Canterbury in the spring quarter of the year 1857 was included the first account of the invasion of this country, at least in any known time, by the disease since then so prevailing and fatal, diphtheria. This disease first appeared in the little village of Ash, and was called the Ash fever. The outbreak was observed and recognized by Mr. Reid, of Canterbury, and was reported to my register by Mr. Haffenden, who collected for me the facts of prevailing diseases from eight medical observers living near to him, of whom Mr. Reid was one. The first facts of a new disease in this country were thus recorded on the spot, which is something even as a matter of history. How such a fact, reported at once to a central government authority, might be dealt with; how promptly a central authority so advised might act in arresting a fatal epidemic at its origin, and what national service might be rendered thereby, you, quite as well as I, can judge!

The fact of a suggestive nature springing from the working of the returns is not less interesting. The labor led me to refer to the returns of sickness sent every week by the medical officers of the Poor-Law districts to their boards of guardians. I found that these returns, over 3,000 in number, which, when they have served their local purpose, are practically worthless, could by the slightest modification be utilized as returns of the sickness of all the sick parochial population under official medical care, and I submitted a plan for such introduction to public approval and to the Government, but without effect. Yet if the plan had been adopted from those three thousand weekly returns, cast away and still cast away, I calculate that 156,000 tables of disease per year would have been submitted to scientific analysis which, since the time when the

suggestion was first made, would have multiplied into 3,276,000 tables, including in each table a record of at least ten times as many particular examples of disease. To what important national uses such an array of facts systematically arranged and examined could have been applied you, as well as I, can judge! And still neither of us can judge effectively, because in dealing with data taken from nature there is always something important to be elicited which never was looked for, and often, too, that something unlooked for is better than that which was specially looked for.

Our Sanitary Institute will do well in continuing to press this scheme for the registration of disease on the Government, and it may greatly assist the work by lending its mind to the best means of collecting the facts on which the weekly reports of disease will have to be based. I might enlarge on this part of my subject, but I should prefer to remain silent until the views of the medical officers of health, now a large and influential class, have been correctly ascertained. My present purpose is served if I have sufficiently directed public attention to the principles of the design.

In the future of sanitary science the politician must come forward more determinately than he has yet done, in order to secure for those over whom he governs three requisites—pure water, pure food, pure air.

The Public Health Act of 1875 deals with the water question, and makes provisions for the local authorities to supply their respective districts, by means of a company, or by independent action. For my part I see no hope of any effective change for the better by these propositions. It is utterly hopeless to trust to companies in a matter of such vital moment. It is equally hopeless to trust to the undirected action of local authorities. If we trusted to such agencies for the collection and delivery of letters by post, does any one suppose that the results of our present postage system would be attained? Yet important as intercommunication by letter is, it is less important than the supply in due quantity and pure quality of that vital fluid which makes up three parts out of four of every human organism, and which is wanted as much by the millions who never receive a letter, as by the millions who do. In this political part of sanitation, the Government must do one of two things. It must either produce a process or processes for pure water supply, and insist on every local authority carrying out the proper method; or it must,—and this would be far better,—take the whole matter into its own hands, so that under its supreme direction every living center should, without fail, receive the first necessity of healthy life in the condition fitted for the necessities of all who live.

By recent legislation we have some security for obtaining fresh animal food, and foods freed of foreign substances or adulterations. The penalties that may be inflicted on those who sell decomposing, diseased, or adulterated foods are beginning to have effect, and much good is resulting. Nevertheless, even here the legal rule falls short of completeness. The inspection of animal food is as yet most unsystematic and imperfect. With all our richness of means ready at command, we have not approached that admirable system for the inspection of animal food which our Jewish brethren, through ages of ignorance and oppression, have managed so efficiently to carry out, and which has entirely saved them from many of the great calamities of disease that have fallen on less careful people. The complete inspection of animal foods, including milk, is a clear piece of sanitary law which, from day to day and hour to hour, must ultimately be enforced.

Imperfect as legislation may be in respect to supply of pure water and food, it is advanced in these directions when the steps it has taken for supplying pure air are brought under observation. There is no practical legislation of any kind on this requisite. The air of our large towns is charged with smoke and impurity. The air of our great factories is charged with dusts which destroy life with the precision of a deadly aim. Dr. Purdon, one of the certifying surgeons under the Factory Acts, reports that in the flax-working factories under his care the carders, who are all females, if they get a carding-machine at eighteen years, generally die at thirty years. Can any fact be more terrible than such a fact,

that a girl of eighteen should have to live by an occupation that will bring her existence to an end in fourteen years, and to that end with all the prolonged wasting, sleeplessness, suffering, incident to the disease—consumption of the lungs. If it were the fate of these doomed workers that at the close of fourteen years' work the majority of them were taken forth and shot dead in an instant, their fate were infinitely better than it is. The heart of the nation would thus be roused, and the law in all its majesty would be put in operation to arrest the progress of the crime and to punish the offenders. Yet, year after year as effective an offence goes on, and because the results of it is hidden in the sick-room there is no arrest of its progress, no punishment for its commission.

In the application of political science to preservation of health not one subject presses more earnestly than the question of the supply of a pure atmosphere to the millions of industrials of these islands. In an inquiry I recently undertook on this matter for the Society of Arts, Manufactures, and Commerce, the facts that came before me were as of a new world. You will find a compact mass of these facts in the lectures I had the honor to deliver before that learned society. Those lectures contain a tithe only of the things seen. I am quite sure that our leading politicians can have no adequate conception of the mental and physical condition of the great industrial classes, or of the need that exists for reconciling those classes to their fate. These truths are plain.

The catechism has failed to satisfy them. Bad air keeps up in them a depraved mental as well as physical state. Their poverty and not their will consents to their condition. In short, as a physician dealing with the physiological and psychological phenomena belonging to a class instead of an individual,—and this is all the difference there is between a politician and a physician,—my diagnosis is that a serious organic state, febrile, fitful, fatal, exists in this part of the nation; that it demands the watchful consideration of all physicians, State and ordinary; and that the sooner the natural cure for it, pure air, and plenty of it, is let in the better for every class everywhere.

All political troubles have a physiological cause. To the Statesman not less than to the physician, physiology is the only true source of knowledge. A society such as ours, therefore, possessing as it does professed physiological skill, may render most important service by tracing out for the legislator the simplest scientific means for removing with atmospheric impurities and by preparing for that sanitary future when men universally shall breath purity even with their freedom.

If any other incentive to action in this direction were required it would be the further fact that all diseases, mental and physical, national and individual, begotten of an impure atmosphere, are transmitted on. The consumption of body, the restlessness of mind are reproduced and gain intensity of development with each generation until practically they inaugurate a distinct racial type of human imperfectedness.

With this topic of legislating for pure air would come in naturally the question of homes for the people and the development of those recent acts which have been passed to meet the necessity. These efforts of the world political can scarcely be over-estimated; but there is one movement which stands before them and which has been singularly overlooked. It is essential that the home of the working man should in every case be cleared of the details of daily work. So long as he is compelled to work in the room in which he sleeps and takes his food, so long his home must be an unhealthy centre, and too often it will be the centre from which infected work will pass out, bearing infection into the homes of the wealthy. A modification of factory legislation by which a free and properly regulated work-room should be within the easy reach of every working man in every crowded centre is a necessity which all sanitary laborers should strive to get supplied. Our Institute has another urgent task before it in the effort to enforce this necessity on public attention.

In the future of sanitary science one more amongst many other reforms of a political character must needs claim important consideration. I refer to the political assistance that must be given to all of us who are engaged in the labor

of quenching the drunkenness of our land. Our best sanitary efforts will fall far short of their deserts until this object shall be achieved. Over the future of sanitary science will be suspended a pall of sorrow until this object shall be achieved. Does any one desire to know how the mortality of the kingdom is modified by strong drink, let him read the knowledge in the State record book which tells that those who sell the destroyer die by it at the rate of one hundred and thirty-eight to the hundred of the whole population. Then, starting from this signal fact, let him trace the influence of the destroyer through all the courses of diseases which, under learnedly obscure names, spring from it and kill from it in all classes of society. Finally, let him reckon up the hereditary evils which are engendered by the same destroyer and the influence of that on the course of disease, and his lesson will be in some measure complete.

I do not think this the occasion to discuss the value of the different political sanitary measures that have been, or are at this time, in the public mind for the repression of the national evil now touched upon. Be it sufficient for me to state two impressions only. Firstly, that every day's experience of the question in various communities where as a teacher of abstaining temperance I am wont to labor, indicates to me that unless the State does come to the aid of the teacher the battle against intemperance must be indefinitely prolonged. Secondly, that if the State itself—doing nothing active in the way of repression, would but determine to cease to legalise the cause of the evil and to make revenue out of the transaction, the labor of the temperance reformer would have the most prosperous season of success presented to his view. Hitherto this has not been considered as a sanitary question. In the future no sanitary student will venture to exclude it from his studies.

The contemplation of the political sanitary future of this kingdom offers many other topics, all of which I must leave in order to devote a few minutes to our subject in its relation to medical science.

THE MEDICAL PART.

The influence which sanitation will exert in the future over the science and art of medicine promises to be momentous. It promises nothing less than the development of a new era; nor is it at all wide of the mark to say that such new era has fairly commenced. The greatest of the world's philosophers, the philosopher whose thoughts cover the world of science as with a garment, I mean Lord Bacon, said of the medicine of his day, that it stood for judgment on quite different merits than did other learned pursuits. "Other arts and sciences," he argued, "are judged of by the power and ability exhibited in the conduct of them by their professors, and not by success or by events. The lawyer is judged by the skill of his pleading, not by the issue of the trial; the pilot by his skill in directing the course of the ship, not by the fortune of the voyage. But the physician can perform no particular act by which his ability can be directly demonstrated, and therefore he is principally judged by the event, which is very unjust. For who shall decide, if a patient die or recover, whether the good or the evil is brought about by art or by accident? Whence," says he, "imposture is frequently extolled, and virtue decried. Nay the weakness and credulity of men is such, that they often prefer a mounte bank or a cunning woman to a learned physician. So the ancients made Esculapius and Circe brother and sister, and both children of Apollo. Hence," he adds, "physicians say to themselves in the words of Solomon, 'If it befall to me as befalleth fools, why should I labor to become more wise?' And therefore one cannot wonder that they commonly study some other art or science more than their profession, because they find that mediocrity and excellency in their own art makes no difference in profit or reputation; for man's impatience of diseases, the solicitude of friends, the sweetness of life, and the inducements of hope, make them depend upon physicians with all their defects."

Had Bacon spoken these sayings in the present day, he had spoken, with one or two exceptional errors, as truthfully as he spoke in his own time. Had he been a physician, he might indeed have gone further than he did. He might have urged his too frequent inadequacy himself to decide whether his own suc-

cess rested, in particular instances, on skill or on accident. He might further have added how oftentimes the cheek of the right-minded physician pales or burns with doubt as he hears his own praises declared for skill which he himself cannot for a moment take credit to his own heart. This has been the fate of medicine until our day. On such fate all the quackeries have flourished; on it all the "pathies" and dogmatic systems of medicine have flourished; on it the idea of cure has found too willing acceptance and belief.

At last a change has come over the science of medicine. With true nobleness of purpose, true medicine has been the first to strip herself of all mere pretences to cure, and has stood boldly forward to declare as a higher philosophy the prevention of disease. The doctrine of absolute faith in the principle of prevention indicates the existence of a high order of thought, of broad views on life and health, of diseases and their external origins, of death and its correct place in nature. The doctrine of absolute faith in curative medicine, of power vested in the hands of a distinct sect or class, and exercised by them as by regal right and without the assistance or interference of those upon whom it is exercised, indicates a low standard of knowledge; a too confiding spirit is the wisdom of a minority; a departure too wide from the safe law of self-preservation; and an ignorance of the avoidable causes of diseases; a blindness and therefore an unnecessary exposure to danger; an overweening and sudden fear of dangers of all kinds little and great, and a hasty and thoughtless pursuit after that mode of rescue from dangers of disease which claims for itself the greatest pretensions and boasts the greatest successes.

It shall remain as one of the glories of medicine that she herself has first seen these truths, and, willing to sacrifice her own interests to truth and light, has put them forward without fear, without reward. In the science of prevention medicine takes in fact all the world with her. The science becomes a political, a social, as well as a medical study. It appeals to every mind. When it once is so set forth it fills all men with its teachings. It models itself into household truths and commingles with the moral and even religious elements of life. Admitted for a season into the household, it steps forth again to find its way into the legislature. It becomes eventually a governing science—a law.

This scientific course commenced, must needs go on. But in its going it must needs also change greatly the old face of medicine, and remove in the change the Baconian reproach. I do not think there is much difficulty in foreseeing what in the main the change will be like.

I need not say that the "pathies" will go. The pathies of all kinds are as dead as door-nails, and wait only to be decently interred in a common grave. In time the word cure will go altogether. It is clear already that there is indeed no such thing. A man born to live through a given cycle lives through it free of disease, unless he be stricken from without. If he be stricken, and by the stroke the natural functions, by the exercise of which he lives, are not so disturbed but that they can swing back again in due order, he may recover; if he be stricken beyond this, he will die. Nature will pursue her course undisturbed by either event. She will make no special effort to kill, and assuredly she will put out no special hand to save. A man may intervene, and may, by knowledge, put the stricken body into such a condition that it may swing back into natural course whereby he will have put it into a condition in which it will not die. This is the very highest development of medical art resting on science. But it is not cure, in the common meaning of that term.

By the progress of sanitary science and by its influence on practical medicine we shall attain these perfect rules of management after the infliction of the stroke of disease; and I do not doubt that the art of placing the stricken under such conditions that they may not die will for ever afford scope for the inventive genius of man. The more immediate triumphs will, however, come in that part of the work which is purely preventive. Down from the skies comes the forked lightning and lays a man prostrate. It is a question for the ages who shall place that man in a condition under which he shall certainly swing

back again into life. But the preventive art that puts up a metallic rod to divert the lightning from other men, that is the present triumph of human skill; skill which, carried to perfection, shall prevent the stroke and put out the second art by removing the necessity for its application.

With the progress of sanitary science we must expect to see preventive medicine taking the ascendancy. Cure will cease, prevention will grow. Humanly-made epidemics, like the great plague of London, which was planted and reared in the rush-covered floors of domiciles saturated with the organic refuse of years, or like the modern typhoid, which is fed by streams of drinking water uncleansed from human excreta, such self-made epidemics will be prevented by simple mechanical skill. Diseases imposed by indulgence in harmful pleasures and appetites, or by physical overwork and shock, will be removed by the effect of moral influences and knowledge of cause; and gradually, I believe, those persistent evils, which, like the lightning-stroke, come without human ordinance or fault, will be placed also under some protecting care, and, if not removed, reduced to a short calendar.

It is felt by some that the medical Sanitarian of the future will have his best efforts thwarted by the forcible excess of life beyond the means that can be found for the support of life, as if life were a mere secondary principle in the universal order. I see no such cause for fear. That in the progress of life on the earth the day will ever come when the earth will not supply food for its people is to my mind pessimism carried into an insane vulgarity. It is clear that man can always reduce to his wants the lives of all animals except man. The question rests therefore on the abnormal increase of man alone. Nature knows that and rules accordingly. Let man remain savage, and, however sensual he may be, he will die fast enough by war, plague, famine, or luxury. In that state he will never overstock the earth, but either grope in solitary places a neglected family, unprotected from all the killing vicissitudes, or will sink into luxurious barbaric decadence. Let man become exalted in life; exalted by communion with noble pursuits; with pursuits of science, art, letters, and cultivation of greatest happiness for the greatest number, and his sensual life will become too subject to the virtue to leave a chance for the danger which a low sensuality sets up as a terror and at the same time a temptation for the vulgar.

I think it my duty to deal plainly with a question which affects so closely the future of sanitation, and to express, from an experience which is confirmed, as I know, by some of the brightest ornaments of my learned profession, that nothing is wanted to correct the danger of over-population but improvement of mental process; nearer communion with the eternal mind in His works; purer artistic education, healthier homes, more rational amusements, and the ennobling influence of a holier life amongst those who assume to be the cynosures of the nation.

On the whole the prospects of medical learning and action will be greatly improved by sanitary advancement. It is possible that fortunes or reputations resting on faith in famous curers will dwindle slowly away, and that not for long will the skill of the physician be valued by the fallacious reckoning of mere results. But in exchange there will be opened to the physician a career in which skill of labor will be exhibited together with results, the results obvious as to their relation to the work, and both, if good, successful beyond praise.

THE SOCIAL PART.

The future of sanitary science in relation to social life generally, its effects that is to say on all classes of the community, promises steady progress. No one who has been actively engaged for the past quarter of a century in sanitary work can doubt this statement. Throughout all sections of the community there is desire to know; and if the legislator will be content not to legislate until he sees that free-will guided by knowledge is in the same train with him—it doesn't matter in which class,—all will go well. The workers in our Sanitary Institute though they be not legislators can, nevertheless, greatly assist Parliament by bringing free-will into harmony with knowledge, and though the distinction does not at first sight stand out, in separating free-will from ignorance and

from those automatic demonstrations of ignorance which are the outward and visible signs of unhealthy habits of life.

The social work that has to be carried out for the future of sanitary science is purely educational. Educational not merely by lectures and books and lessons from books, but by demonstrations of sanitary works, plans, buildings, mechanisms, results of all labors bestowed on the cause. Without venturing on details of this kind which would land me in another address, I may be content to touch on two points, both of vital moment for the future.

The first of these relates to modes of teaching so as to carry the sympathies of the learner and his more refined tastes along with his reason; to attract and charm his senses as well as his intellect. It is said of us sanitarians, and sometimes I fear with some truth, that we would make health hideous. We need not do so; and if the feat has ever been accomplished it is but the work of a "prentice han," that ought to be forgiven. Health truly is beauty in the living evidences of it, and should be so in those inanimate evidences which the builder and the engineer construct for us. I would therefore urge that in all coming sanitary work, theoretical or practical, the sanitarian should call the artist also to his side, and that no design of a sanitary kind should ever be executed in which the hand of the artist does not play its beautifying part.

And if I might suggest so much to the imaginative scholars who live to make life sweeter to the many, I would ask them,—poets, painters, sculptors, players, musicians,—to believe that to render practical even their refined labor is to render that labor more acceptable, more diffusible, more durable.

The second topic relates to those who require first to to be taught the sanitary lessons of the future. I want strongly to enforce that it is the section of the nation which Dr. Farr classes as the domestic, the six million of women of the nation, on whom full sanitary light requires first to fall. Health in the home is health everywhere. Elsewhere it has no abiding place.

I have been brought indeed by experience to the conclusion that the whole future progress of the sanitary movement rests for permanent and executive support on the women of the country. When as a physician I enter a house where there is a contagious disease, I am, of course, primarily impressed by the type of the disease and the age, strength, and condition of the sick person. From the observations made on these points I form a judgment of the possible course and termination of the disease, and at one time I should have thought such observations sufficient. Now I know them to be but partly sufficient. A glance at the appointments, and arrangements, and managements of the house is now necessary to make perfect the judgment. By this is shown what aid the physician may expect in keeping the sick in a condition most favorable for escape from death; and by this is also shown what are the chances that the affection will be confined to one sufferer or distributed to many. As a rule to which there are the rarest exceptions, the character of the judgment is hereupon dependent on the character of the presiding genius of the home,—on the woman who rules over that small domain. The men of the house come and go; know little of the ins and outs of anything domestic; are guided by what they are told, and are practically of no assistance whatever. The women are conversant with every nook of the dwelling, from basement to roof, and on their knowledge, wisdom, patience, and skill, the physician rests his hopes. How important, then, how vital that they shall learn as a part of their earliest duties, the choicest sanitary code. How correct the decision of the founders of the Sanitary Institute, that from the first they should include sanitarians of both sexes as working associates.

To women more than to men this work is new. To women more than to men this work is hard to realize. Naturally more conservative than men they are moved with less haste to tasks of reformation and reconstruction. More sensitive to criticism than men, they are given, at first, to resent, as if it were an insult to past customs and usages to which they are attached, the suggestion of innovation. But these passing difficulties removed, there is in the hearts of women such matchless generosity, such

overpowering love for every device tending to promote the happiness of all things of life, that we sanitarians may indeed be content for the future of sanitary science in its social aspects, if we do no more than win them to our own cause and entrust its details to their ministering spell.

THE LONDON OBELISK AND EGYPTIAN WORK.

From "The Builder."

GREAT London, it is certain, must in time be, if it is not now, as remarkable for the variety of art and architecture it possesses, as it is for its hugeness and extent, and the multitudes who inhabit it. It will in time, too, be as remarkable for the specimens it has every here and there of art foreign to itself, and its ancient remains. These are, indeed, already, of all times and phases of art, and we are, as all know, to have, in no longer time, another not a little remarkable specimen of antique art, and to be placed somewhere,—an Egyptian *obelisk*, Cleopatra's Needle, as it has been somewhat oddly called,—a remarkable monument of the past of things in this world, and indeed a history in itself. It may be, therefore, of some interest to hint at a few things to which this strange monument affords a sort of key; for could all things be told about it, and its origin, and its purposes in the place from which it comes, and all be brought to light, what a strange tale it would tell, and how deep into the old world's history would it lead. We can but hint here at a few things, and but glance at this monument, and read, may be, a little of the inscription on it, and of the things about it. In the first place, it is to be borne in mind that this solitary monolithic shaft is not complete in itself, and as standing alone, as it will do when it forms one of the many wonders of London; but it, like so many others in that strange land, Egypt, from which it comes, is but a part of a great system or whole. It was an item in a temple, or pile of buildings, devoted to the special worship of some pagan deity. Obelisks stood in pairs opposite each other, and must have formed *in situ* objects sufficiently remarkable and significant.

It would be difficult, without a somewhat elaborate plan, to make the position of the Egyptian obelisks quite plain. The temples, including the court-yard in which the obelisks stood, took up a large space of ground. The temples differed, according to their size and situation, in details and arrangement, but the main idea was the same, and in this main idea the most notable, perhaps, was the provision that was made for those long and and imposing religious *processions* in which the men of Egypt took such delight, and which indeed went so far to make up their very religion itself. The successive gateways, the very walls of the temple, the long line of recumbent animal forms, the obelisks, the seated colossi, the pylons or gateways within the temple court or enclosure, the arcades, and the covered or roofed enclosure itself, in which the statue of the god was enshrined,—all seemed to be devised with the one object of giving effect and meaning to those imposing processions and shows. No small impulse, too, to the fine art of Egypt must have come from the sight of those world-notable displays.

All this went to make up the general plan and idea of the Egyptian temple, and all certainly had its material or symbolic use. Ornament, in one sense, and as mere ornament, there was none, for the whole temple and pile of buildings were simply written upon, both within and without, as on its four sides is this very obelisk. Even the capitals of the columns would appear to have been wrought out with a symbolic meaning, and the hieroglyphics to be seen everywhere, and on every otherwise vacant stone or granite surface, were there to be read by all who could read them. In short, an Egyptian temple was an open book, and in passing through it you could but read what was written on its walls

and ponderous blocks of stone or granite.

We have said that by going into details some very considerable space might be taken up, and it is difficult to know what best to select, there being so much that is noteworthy. But perhaps the most striking thing to be noted first in Egyptian undertakings was the enormous size and consequent weight of the blocks of stone which they quarried, and moved from place to place, and lifted into such all but impossible places, and all, too, with such truth and accuracy of fitting and fixing. Sir G. Wilkinson notes not only the immense size of the quarried blocks, but the confined and awkward places from which it was necessary at times to move them. The obelisk, to cite but a single example, transported from the quarries to Thebes, measured no less than 90 ft. in length of one single stone. Nothing seems to have broken the heart, or the "lifting" courage and patient strength of the old Egyptian. Sir Gardner calculated the weight of one single block at Karnak,—the "mighty" Karnak, as well called,—at no less than 297 tons; and this had to be moved to no less a distance than,—incredible as it seems,—138 miles! And even this, great as it is, is by no means the largest or the weightiest of the ponderous masses which the old Egyptian mechanics and workmen contrived to move and lift into place by mere force of multiplied manual labor.

But, perhaps, even more surprising than the lifting and moving of these ponderous blocks are the wonderful artistic precision and accuracy with which they have been placed and fitted together. There are no broken joints, edges, or corners, and the lines are as straight and as true as a straight-edge. We are here noting the simple masons' work only, for the practical difficulties to be encountered and mastered here, though the work may seem to be simple, can hardly be overestimated. Doubtless this obelisk, which is to add to the curiosities of London, will evidence a little of this. Not far from the banks of the river, on which it will be floated here, can it be taken, say the authorities; but, wherever it may be, its workmanship, as workmanship, may be then compared either with that of the granite-work of the Embankment or with any other masons' works, near it. Much more in detail might be noted on the subject of this work of the old Egyptian mason; but there is in the Egyptian sculpture and deep-cut carving room perhaps for even greater wonder at the skill displayed, and at the accuracy of the eye and hand. Obelisks are covered on all sides with hieroglyphics, and the cutting of these was among the many other masterly things which the Egyptian workmen of old could do so well. In the obelisk that is coming here we shall find evidence of this skill; but even without it, this may be seen in the British Museum, where are a number of examples of it, admirable alike for executive skill and inventive and imitative power in rendering of animal and other forms. Indeed, there may be found marvels of executive skill. The hardest black marble has been incised with a truth and finish not to be surpassed. No one, says a practical judge of such matters, who has ever tried to perforate or cut into a block of Egyptian granite will scruple to acknowledge that our best steel tools are turned in a very short time, and require to be retempered. This the French engineers found when the Luxor obelisk was moved from Thebes; and even, it is added, with the best of modern tools, we find considerable difficulty in doing what to the Egyptians would have been one of the least of their arduous tasks. The more this work of antique Egypt is looked at and attentively studied, the more will it become evident how much there is to be learnt from the study of Egyptian monuments.

Whereabouts in London this fine work of Egypt's artists is to be placed is at present uncertain, and how and in what manner it is to be treated is equally so; but let us hope that no attempt will be made to tamper with it in any way, or to do otherwise than place it as obelisks are usually placed when *in situ*, so that nothing may be visible to disturb the impression which such an art work is calculated to make on the thoughtful student and observer. We deprecate, as others have done, the plan sometimes adopted of adding to the pyramidal top of an obelisk, as the French have done at times, and as the old Romans did, and thus destroying that harmony of

lines and simplicity of form which make of the obelisk what it is, and what the Egyptian meant it to be. Its beauty lies in the very simplicity of its outline.

Among many other points to be noted, and for which we have not space, there is one out of which some good ought to come, and it is most instructive. It is the admirable way in which Egyptian artists contrived to treat the surface of their hard granite. It is admirably polished, yet is the color of it not in any way destroyed or marred; and though so polished and smooth, it does not reflect light, like an imperfect mirror. In proof of this the colossal heads in the British Museum galleries should be looked at and studied, and then compared with reference to the injudicious way,—as we take it to be,—in which the granite bases or pedestals, on which they stand, have been treated. These, all of bright red granite, are polished like looking-glasses, and quite kill the heads above them by the violence of the contrast. The best way to polish granite is a problem. We trust that in any attempt (if there be one) to repolish the obelisk, this useful and patent fact may be taken note of, for the incised hieroglyphic all but disappears if care be not taken. One more thought, out of many, and we commend it to engineers. It will depend a good deal on the position in London town of this huge monolith as to how it looks, and what impression it makes, and the Thames Embankment has been suggested as the most appropriate of spots for it, looking at the immense difficulty, if not impossibility, of moving the granite block far from the river. Surely the moving of it ought not to appal modern engineering! It may be a question, indeed, as to what site in London is the best for such an isolated and foreign fragment of art as this, but it may usefully be borne in mind that a very great deal of the impressiveness of the obelisk, when on its own native ground, depends on its close proximity to a building, which it thus emphasises and helps to make complete. In a court-yard would seem to be its appropriate place, and in front of the main entrance to some building. The court-yard of the British Museum would be an appropriate site for this great obelisk; but we rather incline to the Embankment, if a fitting spot with appropriate surroundings can be obtained.

INVESTIGATION OF THE EFFECTS PRODUCED BY LENGTHENING THE HEATING SURFACE IN STEAM BOILERS.

By JACOB T. WAINWRIGHT, C. E.

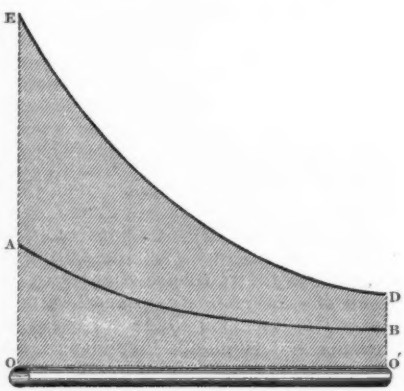
Written for Van Nostrand's Magazine.

It has been found that: "The rate at which heat is transmitted through any part of a boiler-tube varies directly as the square of the difference of temperature" corresponding to that part.

And, without appreciable error, the velocity of the gases of combustion in their passage through a tube may be considered as constant.

From which it will be seen, that if the ordinates of the curve AB, represent the difference of temperature of the inner and outer surface of a tube, the differential equation of this curve may be written thus,

$$\frac{dy}{dx} = -\frac{Cy^2}{V} \quad . \quad . \quad . \quad (1)$$



in which C is a constant for that part of the tube which evaporates the water, and also constant for that part which raises its temperature; and if the feed-water is injected in the same direction as that taken by the gases of combustion, the value for C will be greater than that which it would have, when the feed-water passes in an opposite direction.

V in this equation represents the velocity of the gases of combustion in passing through the tube.

By transposing and integrating in equation (1), the following equation for this curve will be found:

$$y = \frac{V}{Cx} \quad . \quad . \quad . \quad . \quad (2)$$

which is the equation for an equilateral hyperbola.

From this equation it will be seen, that if the ordinates of the curve ED represent the rate of conduction through the corresponding parts of the tube, the equation for this curve may be written,

$$y = \frac{C' V^2}{C^2 x^2} \quad . \quad . \quad . \quad (3)$$

in which C' is a constant, and consequently this is the equation of an hyperbola of the second order.

By applying the rule for quadrature, the following expression we obtain for the quantity of heat conducted through the tube in a unit of time:

$$Q = \frac{C' V Y'^2 l}{Cy'l + V} \quad . \quad . \quad . \quad (4)$$

in which y' represents the ordinate OA, and l, the length of tube.

From which it will be seen, that if in a boiler consuming a given amount of fuel, the length of the tubes be increased proportionally as the smoke area is diminished, the value of V may be represented by $C''l$, (C'' being constant), and consequently equation (4) may be written for this case;

$$Q = \frac{C' C'' y'^2 l}{cy' + C''} \quad . \quad . \quad . \quad (5)$$

From the effect which the value of C has upon this value, it will be seen that it is desirable to inject the feed-water in an opposite direction to that taken by the gases of combustion.

And it will also be seen from the effect which the value of l has upon this equation that it is desirable to lengthen the heating surface at the expense of the smoke area, and that the increase of power and efficiency, so obtained, is proportional to the increased length of heating surface.

It may also be stated, that the efficiency and power so gained, will more than compensate for the mechanical or other contrivances that might be necessary to produce the required draught.

RELATION OF THE RESISTANCE OF MATERIALS TO TRANSVERSE STRESS TO THEIR RESISTANCES TO TENSION AND COMPRESSION.

By WILLIAM KENT, M. E.

Written for VAN NOSTRAND'S MAGAZINE.

IN VAN NOSTRAND'S MAGAZINE for August, Mr. John D. Crehore gives some formulas showing the relation existing between transverse, tensile, and compressive resistances, and then compares results obtained by the application of his formula to transverse stress with the result of experiments by several authorities. The law upon which he bases his formula he considers "axiomatic and inevitable *a priori*," and the coincidences between his theoretical results, and those obtained by experiment are certainly remarkable.

It is but right that I should call attention to the fact that Mr. Crehore's formula for the relation between transverse resistance and tensile and compressive resistances, is substantially the same as the one discovered by myself about two years ago, and communicated by Prof. R. H. Thurston to the American Society

of Civil Engineers shortly afterwards,* in the following words:

"The assumption that resistances vary each way from the neutral surface proportionately with their distance from that surface, is, when coupled with a rejected hypothesis of Navier, nevertheless, not far from the truth in special cases, as may be shown by proper mathematical treatment and comparison with results obtained experimentally." * * * The results of analysis and of experiment give the following values of R in the ordinary formula

$$M=\tfrac{1}{6}RBD^2 \quad . \quad . \quad . \quad (1)$$

for a beam fixed at one end, loaded at the other:

	Cast Iron.	Wro'ght Iron.	Ash.
R—(theoretical)....	32,280	60,000	12,120
R—(experimental)..	35,000	60,000	12,000

13°. This remarkable approximation is thus derived. Suppose a fixed beam, with loaded extremity, and the beam having a depth D, a breadth unity, and a neutral surface situated at a distance Y from the superior surface of the beam. Representing the resistances graphically by the triangles having altitudes $T\,N$, $C\,N$; their measures in tension and compression are respectively $\frac{1}{2}\,T\,N$, $\frac{1}{2}\,C\,N$, and their moments are

$\frac{1}{2}T\,N\times\frac{2}{3}\,Y=\frac{1}{3}\,T\,Y^2$, and $\frac{1}{2}\,C\,N\times\frac{2}{3}\,(D-Y)=\frac{1}{3}\,C(D-Y)^2$.

An early hypothesis of Navier, which seems to have been entirely abandoned by him subsequently, and which has not been accepted by subsequent writers on the subject, make these moments equal. Assuming this to be correct,

$$T\,Y^2=C\,(D-Y)^2 \quad . \quad . \quad (2)$$

$$\text{and } \frac{C}{T}=\frac{Y^2}{(D-Y)^2} \quad . \quad . \quad (3)$$

and, from this expression, we may find the position of the neutral surface, as determined by the assumed conditions. Then, letting B=the breadth of the beam,

$$W\,L=\tfrac{1}{3}\,B\,[T\,Y^2+C\,(D-Y)^2]$$
$$=\tfrac{1}{6}\,R\,B\,D^2 \quad . \quad (4)$$

in which latter expression R is the modulus of rupture, and its value can be found when C and T are known. It will always be of a value intermediate between T and C.

14°. The following are the data and results for the three cases taken: the results are well worthy of examination and record.

	T	C	B	D	L	Y	$D-Y$	WL	R
Cast Iron.....	16,000	96,000	1	1	1	0.71	0.29	5,380	32,280
Wrought Iron.....	60,000	60,000	1	1	1	0.5	0.5	10,000	60,000
Ash Timber.....	17,200	9,000	1	1	1	0.42	0.58	2,020	12,120."

Mr. Crehore, in his article, refers to the above figures as my *experiments*, but says nothing of the formula given with them. I must, however, disclaim the experiments. The experimental figures I took from the *average* figures given in Wood's *Resistance of Materials* page 80. In fact I proceeded precisely as Mr. Crehore has done, viz: framed the theory first, and then tested it *arithmetically*, by comparing it with the results obtained by various experimenters.

Although the results of experiment agree so closely with those derived from the formula, as shown by the figures above given, and also by the long list given by Mr. Crehore, it is not, therefore, safe to assume the absolute theoretical correctness of the formula. The hypothesis of Navier that the sum of the moments of compression is equal to the sum of the moments of tension, which Mr. Crehore adopts because it seems evident as he says, "in the nature of things, or upon the principle of sufficient reason," and which I also adopted, is disputed by late authorities. I will not, however, now discuss its correctness.

If it be assumed that the sum of the *forces*, and not the moments of compres-

* Trans. Am. Soc. C.E., Vol. IV p. 287. Dubois' translation of Weyrauch, Prof. Thurston's Appendix, page 202.

sion and tension are equal, the equations (2) and (3) above given will become

$$T\,Y = C\,(D - Y) \text{ and } \frac{C}{T} = \frac{Y}{D-Y}.$$

Equation (4) of the total moment of rupture will remain as it is,

$$WL = \tfrac{1}{3}B[TY^2 + C(D-Y^2)] = \tfrac{1}{6}RBD^2.$$

in which $$Y = \frac{CD}{C+T}.$$

The *true* theory of rupture by transverse stress has probably never yet been given. The above formula, whether based upon the hypothesis that the moments are equal, or that the forces are equal is only an approximation. A complete theory must necessarily include the effect of the "flow" of the material after it has passed its elastic limit, which will modify the law of proportionality of resistance to distance from the neutral axis, and will vary with every material.

As a statement of conditions upon which a complete and correct theory may some day be based, I believe nothing better can be given than the following remarks of Prof. R. H. Thurston at the Seventh Annual Convention of the American Society of Civil Engineers.

"The ordinary theory, and its resulting equations, in which the resistances of particles to compression and to extension are proportional to their distance from the neutral surface, are apparently sufficiently correct up to that limit of flexure at which the exterior sets of particles on the one side or on the other, are forced beyond the elastic limit.

"With absolutely non-ductile materials, or materials destitute of viscosity, fracture occurs at this point. But, with ordinary materials, and notably with good iron, low steel, and all of the useful metals and alloys in common employ, rupture does not then take place.

"The exterior portions of the mass are compressed on the one side, offering more and more resistance nearly, if not quite, up to the point of actual breaking, which breaking may only occur long after passing the elastic limit. On the other side, the similar sets of particles are drawn apart, passing the elastic limit for tension, and then resisting the stress with approximately constant force, 'flow' occurring until that limit of flow is reached, and rupture takes place.

"Fracture may occur under either of several sets of conditions.

"A. The material may be absolutely brittle. (*a.*) In this case, the elastic limit and the limit of rupture coincide for both simple tension and simple compression. The piece will break with a snap when, under flexure, either limit is reached. (*b.*) Or, it may happen that the limit is reached simultaneously on both sides.

"B. The material may be slightly viscous. (*a.*) The flexure of the piece will produce compression or extension, or both, beyond the elastic limit before rupture, giving three sets of conditions to be expressed by the formula. (*b.*) The increase of resistance, after passing the elastic limit will not be similar for both forms of resistance, and each substance will probably be found characteristically distinguishable from every other. (*c.*) It would appear from experiments already familiar, that the resistance to compression will frequently increase in a very high ratio as compared with that to extension, thus swinging the neutral surface toward the compressed side, and probably sometimes approximately to the limiting surface, with very hard and friable substances, thus bringing about something like a correspondence with 'Galileo's theory.' This, I presume, does not often happen.

"C. The material may be very ductile or viscous.* (*a.*) In this case the phenomena of flexure and rupture will be as last described, but of exaggerated extent and importance. (*b.*) The resistances to extension and to compression as developed in this case, will be approximately, or accurately, those observed in experiments producing rupture by direct tension and by direct compression. The neutral surface will be determined in position by the ratio of these ultimate resistances."

An interesting historical sketch of the various theories which have been advanced upon the relation of transverse to tensile and compressive resistances may be found in Prof. Wood's *Resistance of Materials*, Chap. III. Recent discussions

* Trans. Am. Soc. C.E., Vol. IV p. 284.

of the subject are published in the Transactions of the American Society of Civil Engineers, Vols. III and IV. The latest theory is given by Mr. D. K. Clark, in his *Manual for Mechanical Engineers*, in which he introduces a new element, called "Diagonal resistance." It is, we think, far from being a correct theory, and has been severely criticised by writers in the London *Engineering*. It is to be hoped that the discussion will be continued, and that some one will through it be led to determine the true theory, and settle forever a question which has vexed engineers since the time of Galileo.

THE LATEST CONTRIBUTIONS TO OUR KNOWLEDGE OF THE SEWAGE QUESTION.

From "The Builder."

It is with some feeling of disappointment, not to say of perplexity, that we endeavour to skim the cream of the recent five days' debate at the Institution of Civil Engineers on the subject of the sewage question. Very much in that debate was of a nature to command respect. New facts were cited. Old facts were illustrated or proved. Men of undoubted eminence in different departments of science addressed the meeting. The dissatisfaction to which we refer arises, not so much from any want of earnestness, skill, or perseverance, but from the fact that, in spite of all, we seem to get no further forward in the matter. The sittings of the 6th, 13th, 20th, and 27th of February, and of the 6th of March last, were occupied in reading and discussing a paper by Mr. C. Norman Bazalgette on the sewage question. But the sittings of 2nd March and 4th and 11th April, 1876, had been occupied by the reading and discussion of papers by Mr. G. R. Redgrave and Mr. W. Shelford on branches of the same subject; and no doubt if we were to trace back the accounts of the Transactions of this body year by year we should find that these were but a few out of many similar debates. But when we ask what is the upshot, we find little more than a record of the experience of contradictory opinions, urged over and over again, among which the bewildered engineer is as much at a loss as ever. We confess that the most distinct conviction which a study of the debate has brought with it is one to the effect that we have too much undervalued certain institutions of the country, on which of late no small degree of discredit has been cast. We mean those rough and imperfect, but yet practical, modes of arriving at a decision on the main gist of a dispute, which we are in the habit of effecting by the verdict of a jury in certain cases, and by a division in either House of Parliament on legislative matters. If some sort of division were called for as to the "conclusions" with which each writer terminates his essay, so that the world should understand that such conclusions did, or did not, meet the acceptance of the majority of the Institution, we might gain much. Not that this would be all that is requisite to make these debates as valuable to the country as they are interesting to those who take part in them. We want the classification so roughly effected in our law courts as to matters of fact and matters of law. We require some sort of seal to be set on the veracity of the statements of fact laid before the Institution; and then should follow a judicial summary of the scientific bearings of these facts. Indeed, we want a series of decisions on specific resolutions, and a competent summary of the upshot and tendency of these conclusions. If this were done, the progress might be slow, but still it would be progress. Now, the movement may be apparently more rapid, but it is, after all, only beating time. No ground is gained. Some of those conclusions as to which we confess we were ourselves disposed to hold that a general acceptance might be predicted, were more ably and more fiercely contro-

verted in the recent debate than probably at any previous time. Mr. Bazalgette referred to the conflicting opinions expressed by the Rivers Pollution Commissioners, by the speakers at the Conference of the Society of Arts, and by the sub-committee of the British Association, and the number of disputants might have been almost indefinitely increased. But in his classification of treatment with chemicals, which if it had been altogether new would have been of permanent value, this gentleman took no notice of the tabulated statement of the comparative outlay on certain chemical processes which was laid before the Institution by Mr. Shelford last year. In his "conclusions," Mr. Shelford then gave exact statements, which must be capable of either verification or disproval. In the former event, a reference to points thus established would have saved much time, both in the composition and the reading of Mr. Bazalgette's paper, and in the discussion which followed. In the latter case, it is no less important that the disproof should have been made clear, so that no unwary person should have been led to rely on the paper last referred to, as embodying conclusions accepted by the main body of the profession. Without offering, at the moment, any opinion as to the actual soundness of any of the views expressed, it is obvious that the form of Mr. Shelford's paper is more exact than is that of Mr. Bazalgette; and that, as a matter of convenience, the substitution of a more general for an exact statement, unless to the effect of proving the latter to be made in error, is not advance, but retrogression.

The "conclusion" arrived at by Mr. Shelford is to the effect that however difficult, and even impossible, it may appear, there can be no doubt that any treatment of sewage which falls short of its profitable application in agriculture, fails to solve the sewage question. The "conclusion" of Mr. Bazalgette is "that no profit must be expected from the cultivation of crops by the sanitary authority, and only a moderate one by the farmer" Mr. Bazalgette further lays down the "conclusion" "that towns situated on the sea coast, or within the tidal range of rivers, should avail themselves of the means of outfall thus presented, as affording the most economical and efficient means of dealing with the sewage." Mr. Crookes, F.R.S., a scientific man of no small note, contends, "were the whole of the sewage to be discharged into the sea, the supply of fixed nitrogen could not stand the drain very long. If, then, the community continued to waste the supply of available nitrogen, the time might come when there would be neither bread nor beef,—and not even gunpowder." It will be seen that if statements of such antagonistic character are published side by side, or even year after year, without any indication of the degree of truth which, in the opinion of the majority of the debating body attaches to either side, the value of the debate, as such, is *nil*, and we merely have an opportunity for the statement of a number of incongruous private opinions, which no one has the power to seize and weld into the compact metal of truth.

It is obvious, in our opinion, and we have before now expressed that opinion, that the first thing to be done in order to arrive at any positive scientific rules with reference to the disposal of the sewage of our towns, is to ascertain whether the product has, in any given case, a practical value. That it has a theoretic value we are ready at once to concede to Mr. Crookes and those who agree with him. As to the maximum money value there is not so much dispute. The practical question is, how many shillings will it cost to extract 5s. worth of value from the corresponding quantity of sewage, while at the same time the offensive and dangerous character of the substance is effectually destroyed? Now, without assuming to speak too positively, it cannot be denied that the general upshot of all experience to that effect is, that no economic treatment, generally applicable, has as yet been discovered. Something to this effect is Mr. Bazalgette's first conclusion: "No chemical process can efficiently deal single-handed with sewage, but must be assisted by subsequent natural or artificial filtration of the treated sewage; and therefore no chemical process *per se* should be adopted for the purification of town sewage." With certain qualifications, we are disposed to accept this conclusion of Mr. Bazalgette. But what the public will ask is,—Is this the conclusion of the

Institution of Civil Engineers, or the mere opinion of an intelligent barrister?

Now, we must confess, with all deference to the many able men who have joined as combatants in this part of the fray, that we are at a loss to understand how it is that this primary question should be at the present time in doubt. We do not make this remark by way of introducing any idea of our own as necessarily the right one, but in the hope of indicating such a mode of inquiry as must lead all impartial investigations into the way of truth. Mr. Crookes tells us that the population of the United Kingdom waste, at the rate of ½ oz. per unit, 445 tons of fixed nitrogen daily, the market value of which is £44,500., or yearly £16,000,000. Dr. Voelcker, who travelled all over Belgium with a view of getting special information from the town authorities, on the other hand, knows of no process, nor believes in the existence of any, for the profitable manufacture of portable manure from sewage. He instances how, in what is called Campbell's process, the addition of £63. worth of chemicals to the million gallons of sewage produced a precipitate of nitrogenous matter of the value of £65. 5s.; but that the cost of the precipitation process for the quantity was £90., which gave a deficit of £1. 2s. 10d. on every ton of manure produced.

Until such extraordinary divergencies in the statements of experts can be explained, it is evident that we have yet to learn the A B C of the sewage question. And it seems to us that the difference between the theoretic and the practical view may be thus investigated. The ingredients of sewage consist of four groups,—namely, water, organic substances, gas, and minerals. Of these water forms nearly 93 per cent.; and the object of the sanitary engineer is to send forth that water chemically pure from his works. Organic matter forms 4.62 per cent. (We are speaking not of dilute sewage, but of pure excreta; for sewage there will be a dilution of from 90 to 100 fold, exclusive of any further dilution from storm water.) It is here that the main source of danger lurks. It used to be considered that the organic matter was a valuable food for plants. It is now, we apprehend, generally known that the reverse is the case. Plants cannot assimilate organic albuminous refuse, and the only thing which can be done with it, to prevent its evil results, is to burn it,—whether that is done by filtering through porous soil, when it becomes gradually oxidised, that is to say, burnt, or in any other manner. No doubt exists as to the fact that this 4.62 per cent. is the main cause of our perplexity and danger, and that its resolution into other forms by combustion is the first object of the engineer. There remain .70 per cent. of mineral, and 1.8 per cent. of gaseous matter. The mineral matter has been derived, by the process of vegetation, from the soil. Its presence is necessary for organic growth. Its return to the soil is a matter highly desirable, or even necessary, if the soil be defective in the elements in question. But here lies the doubt. The mineral elements are lime, magnesia, silica, oxide of iron, soda, potassa, sulphur, and phosphorus, the latter being found in the form of phosphoric acid. It is the last substance alone of which the rarity is such that the return to the soil of the quantity actually abstracted by the animal growth of the crops is an object of extreme importance. It is rather by the mixture of soils, as in Wales, where lime is in many places the only manure employed, than by the return of sewage to the land, that all these minerals, except the phosphorus, and perhaps the sulphur, can be best supplied. So that the real question here is, in what manner can we most economically supply to the growing crops the phosphorus or phosphates which they require?

With regard to the gaseous elements of sewage, a more undetermined, and therefore a more serious, question arises. That vegetable growth requires the presence of nitrogen is well known, as it also requires that of carbon, oxygen, and hydrogen. No doubt, all these gases exist in sewage. But the question which is not as yet absolutely clear is, in what manner, and from what substances, does the plant derive its requisite supplies of these elements? The general view is, that it is from those combinations which are known to exist, and to occur more or less freely mingled with the atmosphere, viz., water, carbonic acid gas and ammonia, that plants derive their food. If this is exclusively the case, it can only

be the nitrogen which exists in sewage in the form of ammonia that is of agricultural value. The quantity of this gas found combined in nitrates or nitrites must be left out of sight. Further, it is by no means established that, supposing the other conditions favorable to growth to be present, the plant is unable to supply any deficient amount of nitrogen from the air itself, of which that gas forms so large a portion,—a portion, moreover, not held in chemical combination, as it is in ammonia, but—as is usually considered—in mechanical mixture.

It results that the only element of the food of plants of which we can be tolerably clear that we annually deprive the land by continued cultivation, without return of the manure of the animals, including man, fed on its area, is the phosphorus, in some form, and perhaps the sulphur. Most of the other elements we can readily supply, and at far less cost, than is involved by any process of extracting them from sewage. The phosphates we have also different methods of supplying, of which, it seems very probable, the extraction from sewage is the most costly and unmanageable. We are not at present speaking of the direct application to the soil of effluent sewage. That is an independent question. We are inquiring into the general subject of the extraction from the sewage of great towns of those elements of agricultural value which are not only essential to the growth of plants, but which cannot be more cheaply obtained by any other method, or from any other source, than by extraction from sewage. It is here that so much confusion is allowed to linger. People are too apt to consider the result of a combined process to be favorable, while it might be the case that the result of two independent processes,—one simply a matter of cost, but the other remunerative, would be far more advantageous. It is a consideration of this nature alone, in our opinion, which is adequate, if not to put Dr. Voelcker and his friends in perfect accord with Mr. W. Crookes and his supporters, yet at all events to explain to a third person how it is that two such eminent men take such opposite views, and to show how much truth there is in each of them.

We shall, therefore, be extremely glad if we are able to elicit, from those persons who are able to speak with authority on the subject, a statement of those points on which general, if not unanimous, accord exists as to the main principles which most regulate the satisfactory disposal of sewage. We think we may assume as a primary and cardinal article of accord, the position that all water which has formed a portion of sewage, or come in contact with sewage, must pass through the earth before it is to be regarded as disinfected; and that whatever other methods of disinfection be employed,—filtration, precipitation, or what not,—this earth process is indispensable. We may add, as corollaries to this proposition, that the purifying value of different kinds of earth varies; that experiments have been made on this subject, but that contention still exists as to the value of the results obtained; that further experiment and information are highly desirable; that experience up to this time points to a silicious sand, of sharp grit, as the most valuable sort for the purpose of oxidising sewage; and that the flow of sewage through the stems and roots of plants alone, or through herbage on the surface of the ground, is entirely inadequate to purify the effluent water.

The second proposition, as to which we hold that an almost unanimous assent will be given, is that the organic matter contained in sewage is a positive source of mischief and danger; and that until it be in some way combined with the dose of oxygen which it has lost, it is pernicious, and not useful, to vegetable as well as to animal life. A corollary of this proposition is the statement that the oxidation of this matter at the very earliest moment possible, is the great object which the engineer has to set before him. For example, if cost would allow, the purification of sewage at the very source by the abundant supply of a material containing a large over-dose of oxygen, such as is the case with a well-known disinfecting fluid, would set this matter to rights without more ado. We know, then, what to do, were expense no object, and our efforts should all tend in this direction, while attempting at the same time to exercise practical economy.

Thirdly, it would be of extreme service to all sanitary engineers and agents if chemists and physiologists could arrive at a clear and luminous knowledge of the mode in which plants assimilate their food. The chemical elements of the plants are known. The fact that certain mineral elements, such as silica, are more or less freely present in different plants of the same species, according to the nature of the soil in which they grow, is most important as bearing on this part of the inquiry. But the main point which it is needful here to ascertain is that of the manner in which plants imbibe their gaseous food. Do they at any time absorb oxygen from the atmosphere by their leaves or by their flowers, or only pump it up, as an element of water or of carbonic acid gas, from their roots, and evolve it from their surface? Again, how do they behave as to the other gaseous elements? and in what manner do they assimilate carbon? The general use of vegetable mould as manure points to the view that there is a power in the growing plant to assimilate carbon which is not,—when brought into the proximity with the roots,—in the form of carbonic acid. This point should be made certain. We shall then be more able to ascertain whether nourishment of a nitrogenous kind is derived from the presence of nitrates in the soil, from that of ammonia, or from the nitrogen of the atmosphere. Remembering the curious phenomena of what is called the nascent action of gas (as in the case of the action of hydrogen on spongy platina), and looking at the use of leaf-mould or even of soot, as a manure, there is much reason to suppose that there may be a power in the spongioles of the roots of plants to dissolve solid compounds, or to form new liquid and gaseous compounds, so as to feed their fibres on particles of carbon brought near them in a solid state. The same, it might be inferred, may be the case as to nitrogen. On the other hand, if the opinion that the plant absorbs a main part of its nutriment from the atmosphere directly be correct, it is an argument in favor of the absorption of nitrogen in the same manner. And this is not a mere pedantic bit of trifling, or physiological puzzle; it is a definite, practical inquiry, of a direct economic import. If the plant absorbs all its nitrogen from ammonia, the only value of the nitrogen in sewage will be that due to the quantity of that element which, in the state of ammonia, can be brought into contact with the plant. In that case the nitrates have no agricultural value, except in so far as their mineral elements are concerned, and that is hardly worth discussing. If the plant can derive nitrogen from nitrates present in the soil, the value of that portion of the constituent parts of sewage may be considerable. But if the plant can absorb the comparatively small quantity of nitrogen which it requires in different modes, and can take it from the atmosphere, whether it be present in combination, in the form of ammonia, or in mixture, in the form of atmospheric air, it is plain that we may grossly overvalue the nitrogen present in sewage, and thus be only incurring useless expense by endeavoring to utilise it as manure.

We cannot urge too much the importance of deciding the question of the value of the gaseous elements in question as applied to the growth of plants. No discovery can tend so powerfully to the settlement of the sewage question as this. There is a very strong, and, we believe, a spreading conviction, that the advance of the sanitary question has been more hampered and throttled by the idea that sewage is to be utilised than by anything else. The idea is certainly becoming more and more general that what is wanted is not utilisation, but destruction. Until this fundamental doubt be settled, it is idle to hope for any material progress in the matter. We may question whether the plan of throwing the sewage into rivers or into the sea be a good one or a bad one. But it is one thing if we have only to decide whether we had better, at a given cost, establish a set of outlets of this nature on our shores, or, at another cost, oxidise —that is to say, burn—the offensive matter at home; and another thing if we have to deal with a man who, with all the authority of Mr. Crookes, tells us that throwing into the sea, or burning on the shore, is alike wasteful, and that by adopting either process we are slowly depriving the country of its natural wealth. What we desire is that there should be independent, impartial, and combined scientific effort to remove such

a question from the limbo of doubt and of floating opinion altogether, and to place it on the sound basis of scientific truth. And in saying this, we think that it is as much in the interest of the high and honorable position of the truly scientific man, as it is in that of the advance and discovery of truth, to add that the experts whose opinions will be most valued will be those who are entirely disconnected with any process or project of a financial nature. We neither intimate nor believe that any scientific man, worthy of the name, would be consciously biased in giving a chemical or an engineering opinion by personal interest. But those must have a very imperfect knowledge of human nature who are unaware how strong is the influence of unconscious bias. To that influence, in nine cases out of ten, all differences of opinion in matters of scientific research are primarily due. To that cause, to go no further, must be ascribed much, if not all, the reason of the opposite views taken by such men as those to whom we have referred. What is requisite is, that scientific investigation should be purely and exclusively scientific,—that the result which is to come from the test-tube or the receiver should be a matter of indifference to the analyst.

Thus only shall we really advance towards common accord,—by discarding assumptions, by abandoning all erroneous ideas, by laying a firm basis as to what we know, and by thus advancing, step by step, with equal certitude, to that which we desire to know. With the ground quite clear as to the utmost financial advantage which, under given circumstances, can be derived from the chemical elements of which we have spoken, we shall be in a position to discern the cost at which such advantages are to be obtained, and thus to strike the balance of profit and loss, and to understand in which direction to put our shoulders to the wheel.

RESULTS OF EXPERIMENTS ON CONTACT RESISTANCE.*

By Professor W. A. NORTON.

From "The American Journal of Science and Arts."

The experiments here referred to were undertaken with the view of determining the law of the diminution of the minute distance between two surfaces in contact, with the increase of the contact pressure; and its dependence on the extent, condition and nature of the surfaces in contact. Rectangular pieces of various substances $\frac{1}{8}$ inch in thickness, $\frac{1}{4}$ inch in width, and of suitable length for clamping were used in the experiments. The lower piece was clamped to a horizontal iron bar, which was firmly clamped to the vertical pillars of the testing machine used in my former experiments on deflection and set, and was also firmly propped directly beneath the point where the contact occurred. The other piece, $\frac{3}{4}$ inch in length, was keyed to the under surface of the lever used in the same experiments, at the farther end. The weights were placed on a scale pan resting above this on the lever, and vertically over the surfaces in contact. The depressions of this end of the lever were determined by means of a micrometer screw, which gave the equal elevations of the other end to within $\frac{1}{40000}$ of an inch. The firmness of the lower contact piece and its support was frequently tested by causing the weights to press directly upon it, without the intervention of the lever. The small thermal error of the apparatus was carefully determined and allowed for whenever any perceptible change of temperature occurred during any single series of experiments; but the precaution was taken to secure a nearly uniform temperature during the progress of the experiments. The weights employed, in the more precise determinations, ranged from two ounces to twenty-four ounces. The apparent surface of contact varied from $\frac{1}{32}$ of a square inch to a mere point. The touching surfaces were in some instances

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smooth, in others rough; and in the contact of plate glass with plate glass, highly polished. The decrement of contact distance was noted whenever a weight was put on, and the increment when the weight was removed, and in general the average of the two taken. By this means the thermal error, when the rise or fall of temperature was uniform, would be eliminated; as well as any error that might result from a change in the coefficient of the contact resistance, induced by the pressure and not passing off when the weight was removed. That errors from irregular variations of temperature, irregular variations of the coefficient of molecular resistance, and accidental causes, might be in a great degree eliminated, the mean of a considerable number of separate determinations was obtained in each case. A comparison of these means for sets of experiments differing in number, showed that the irregular and accidental errors were generally small. The initial pressure was the same in the different sets of experiments, and was very slight—being barely sufficient to secure a decided contact.

When a weight was applied the resulting diminution of the contact distance was generally greater than the increase that resulted from the removal of the weight. The reverse very rarely occurred; though the increment was sometimes equal to the decrement. It therefore generally happened that there was a slight contact set when the pressure was withdrawn. These facts show that the application of the contact pressure was generally attended with a diminution of the coefficient of molecular resistance at the surface of contact. When the pressures were renewed at short intervals, the contact set at first observed was generally maintained, and often increased.

The following table gives the diminutions of the contact distance obtained with the several weights, 2 oz., 4 oz., 8 oz., 16 oz., and 24 oz. It is to be understood that the numerical determinations given in the table are the means of a number of individual determinations. It thus happens that the decimals are carried beyond the reliable reading of the apparatus. The mean results of different sets of experiments are given in two instances. The apparent surface of contact was about $\frac{1}{32}$ of a square inch, except in the case of the contact of a flat surface with a round surface of sharp curvature, in which the area of contact was too minute to be estimated.

	Iron on Iron.	Same.	Average.	Iron on Iron.	Same.	Average.
	Surf. smooth	Same.		Flat surface on round surf.	Same.	
	In.	In.	In.	In.	In.	In.
2 oz.	0.000170	0.000162	0.000166	0.000165	0.000162	0.000163
4 "	.000250	.000285	.000267	.000240	.000275	.000257
8 "	.000340	.000325	.000332	.000320	.000275	.000297
16 "	.000450	.000425	.000437	.000410	.000425	.000417
24 "		.000500	.000500			

	Iron on Brass.	Brass on Brass.	Brass on Brass.	Plate glass on plate glass.	General average.	Reliable average.
	Surf. smooth	Surf. smooth	Surf. rough.	Surf. polished.		
	In.	In.	In.	In.	In.	In.
2 oz.	0.000167	0.000170	0.000175	0.000170	0.000169	0.00017
4 "	.000256	.000250	.000267	.000212	.000251	0.00025
8 "	.000335	.000256	.000250	.000294	.000294	0.00029
16 "	.000412	.000410	.000400	.000350	.000404	0.00040
24 "	.000500	.000500	.000500	.000475	.000493	0.00049

On examining this table it will be seen,

(1.) That the diminutions of contact distance were very nearly the same, whatever was the nature, or condition of the surfaces in contact.

(2.) That they were nearly independent of the extent of the surface in contact; since they were nearly the same when the surfaces touched in a mere point, as when the surface of contact had an extent of one-fourth of an inch by one-eighth of an inch.

(3.) That the diminution of contact distance for an increase of one ounce in the pressure, was nearly inversely proportional to the pressure. The fractions of an inch that would answer to this law are as follows: For 2 oz. 0.00017 in., for 4 oz. 0.00025 in., for 8 oz. 0.00033 in., for 16 oz. 0.00041 in., for 24 oz. 0.00046 in. These values differ but little from those given in the table as the reliable averages. The only material discrepancies occur in the results for 8 oz. and 24 oz. Now the table of results shows that in a few cases some cause was in operation to reduce the diminution of contact distance for 8 oz. to nearly the value observed for 5 oz. The same tendency was also often manifest in the individual experiments. If we reject the results for 8 oz. in these cases, that occur in the table, the average diminution of contact distance for a pressure of 8 oz., comes out 0.00032 in., and the discrepancy is reduced to 0.00001 in. Again the experimental result for the case of 24 oz. is 0.00003 in. larger than the law above stated calls for; but the individual micrometer readings were liable to this amount of error, and hence if the support had been depressed by this amount, by the 24 oz. weight, it would have escaped detection.

That the law of diminution of the contact distance which has been stated is very nearly, if not the exact law of Nature in the case, may also be inferred from the fact already stated, that the variation of contact distance is nearly if not entirely independent of the extent of the surface of contact. For if the contact area be diminished in any ratio, say 2 to 1, under the pressure of the same weight the pressure at each individual point of contact would be doubled, and the increment of pressure at each point, resulting from an additional weight of one ounce, would also be doubled. Now if we suppose the law, just referred to, to hold good for a given surface of contact, the diminution of contact distance at each point should be inversely proportional to the pressure on it, and therefore be half as great for the same increment of pressure there, as in the case of the larger area of contact; but in fact the additional pressure at a single point, resulting from an additional weight of one ounce, is doubled, and hence the diminution of distance should be the same as in the case of the larger area of contact.

We may conclude, therefore, that in the contact of surfaces, the force of molecular repulsion, in which the force of contact resistance consists, conforms in its variations very nearly, if not exactly, to the law that the decrement of the distance between the molecules, for the same small increment of pressure, is inversely proportional to the effective pressure by which the molecules are urged into closer proximity. If then we suppose the distance between the molecules to be denoted by x, and the effective molecular repulsion by r, and observe that x is a decreasing function of r, we may put $dx=-m\frac{dr}{r}$. This gives, by integration, $x=c-m$ log. r; or $x=m$ log. $\frac{n}{r}$, in which n is a new constant. It appears then that the curve of the effective molecular repulsion, which resists contact pressure, is the logarithmic curve.

The force of molecular contact repulsion cannot be identical with the effective repulsion in operation in the interior of bodies, when they suffer compression; for the same force of pressure produces a vastly greater diminution of molecular distance at the surface of contact than in the interior of bodies. Thus, in our experiments, a pressure equivalent to 30 lbs. to the square inch, diminished the contact distance by $\frac{1}{2500}$ of an inch. This pressure operating on an iron rod one inch in length would compress it $\frac{1}{800000}$ of an inch. The distance between its individual molecules would be reduced $\frac{1}{800000}$ part. This is immeasurably smaller than the observed diminu-

tion of contact distance; and therefore than the diminution of molecular distance at the point of contact, if the decrease of contact distance consisted simply in the closer approximation of the contiguous molecules of the two surfaces. It is not improbable, however, that it consists in part in a compression of a thin layer of molecules at the surface, having a comparatively small coefficient of elasticity. If such a layer have a thickness as great as $\frac{1}{100}$ of an inch, the compression it would receive from a pressure of 30 lbs. to the square inch, would still be 32000 times greater than a layer of the same thickness in the interior of a mass of iron would experience from the same pressure.

We must conclude, therefore, that the force of molecular contact repulsion has, for the same diminution of the distance between the molecules, an exceedingly feeble intensity in comparison with that of the internal molecular repulsion. It must operate then at greater molecular distances; and accordingly the range of its action must lie outside of that of the attraction of cohesion. In confirmation of this conclusion it may be stated that in none of the experiments was any evidence obtained of an attraction between the surfaces, operating outside of the contact distance.

It would seem, then, that the experiments discussed have served to establish the existence of an effective force of molecular repulsion, in operation at the surface of contact of bodies, whose sphere of action is external to the range of the attraction of cohesion for the same molecule, and which has a much feebler coefficient of intensity than the effective molecular repulsion exerted within the sphere of this attraction. They have also made known the law of variation of this force with the change of molecular distance, and shown that its coefficient of intensity is the same, or nearly the same, for the different substances used in the experiments.

THE MECHANICS OF VENTILATION.

By GEO. W. RAFTER, C. E.

Written for VAN NOSTRAND'S MAGAZINE.

I.

FIRST PRINCIPLES.

THE necessity for change of air in inhabited spaces is rendered evident by considering the sources of contamination: they are,

a. The production of carbonic acid by respiration.

b. Increase of moisture from the same cause and exhalation from the body.

c. Organic impurities from the bodily exhalations.

d. Heat thrown off from the occupants and from the lights at night; and

e. The production of carbonic acid from the lights.

The object of ventilation, therefore, is to remove foul air and substitute fresh air in place of the foul air so removed.

Ventilation, then, reduced to its simplest terms, is a matter merely of the movement of bodies of air; and, since, air is a substance possessing weight, the whole question is one of mechanics, and is in the fullest sense susceptible of numerical computation.

This being granted further progress is comparatively rapid, consisting (1) of the development of the laws of air in motion, and (2) in the application of those laws to the removal of foul air, and the consequent supplying of fresh air in buildings and closed structures generally.

Before entering upon the strictly mechanical portion of the subject, it will be well to consider briefly (1) the nature and extent of the contamination rendering change of air in inhabited confined spaces necessary, and (2) the collateral head of ventilation—warming.

SOURCES OF CONTAMINATON.

a. Production of carbonic acid by respiration. When air passes into the lungs it undergoes a chemical change whereby a certain amount of carbonic acid is produced. This when expired

adds to the impurity of the air, and were its production in respiration continued a sufficient time in any tight space, the air would ultimately become so impure as to produce death. It has been found by experiment that air in its normal condition contains in all parts of the world an amount of carbonic acid equal to from three to four volumes in 10,000. That some must exist in the air will be readily inferred by considering that respiration and combustion always produce this gas. The amount, however, was only determined by the researches of Regnault, who analysed air from many localities. When carbonic acid is the only impurity from eight to ten volumes in 10,000 may be respired without serious inconvenience, though six volumes in 10,000 is taken as the limit of good ventilation for reasons to appear hereafter. An adult produces 0.6 of a cubic foot of carbonic acid per hour.

b. Increase of moisture from respiration and bodily exhalations. The amount of moisture present in the air at different times varies greatly. Observation shows that there are certain amounts of moisture which when exceeded lead to a rapid deterioration of the air. From 4.5 to 5.0 grains in a cubic foot of air at 60°–62° is the limit of good ventilation.

c. Organic impurities from the bodily exhalations. There is always present in illy-ventilated apartments, especially school-rooms where children from the poorer classes are present, a certain unpleasant smell which a medical friend of large experience in ventilation characterises as "cheesy." In simple justice it must be said that this smell is by no means confined to school-rooms of the kind indicated. Excellent examples of it are frequently noticed in concert halls, theatres, lyceums, and in private houses where even an intimation of uncleanliness would be rank injustice. Organic analysis has thus far been unable to do more than detect the simple presence of this ill-smelling enemy of the human race. It has been found, however, that a rapid increase of the organic impurity takes place when carbonic acid exceeds six volumes in 10,000, or when moisture rises above five grains in a cubic foot of air at 60°–62°. The reason for the limit of carbonic acid and moisture as above given is therefore apparent.

d. Heat thrown off from occupants and from the lights at night. It has been determined by observation that an adult gives out 470–490 units of heat per hour, and that an ordinary sperm or tallow candle gives out in burning one hour substantially the same amount. The specific heat of air is 0.238 when water is taken as unity, consequently the heat from a single person or single candle in one hour would raise 1974 lbs. of air 1°, or, since, a pound of air at 60°–62° equals thirteen cubic feet (exactly 13.09 at 60°) we have heat enough to raise 25662 cubic feet 1°. It will be shown, hereafter, that 2000 cubic feet of air per hour is a fair allowance for an adult. It follows that every person, and every candle, gives out heat enough in an hour, to raise the supply per hour, for each person, from 12° to 13°. If a room were constructed of a capacity equal to say ten persons, and the air supply exactly regulated to give each 2000 cubic feet per hour, the bodily heat alone would increase the temperature of the supply from 12° to 13°.

A large majority of buildings are now lighted by coal gas; we will, therefore, consider the amount of heat produced by the combustion of a cubic foot, having ascertained which it will of course be easy to calculate the elements for rooms of any given capacity and number of lights.

The average of a large number of analyses of coal gas of from twelve to eighteen candle power is as follows:

Hydrogen	=	43.76
Marsh Gas	=	40.47
Carbonic Oxide	=	5.94
Olefiant Gas	=	6.58
Nitrogen	=	1.05
Oxygen	=	0.47
Carbonic Acid	=	0.75
Aqueous Vapor	=	1.00
		100.02

The specific gravity of sixteen candle power gas is approximately 0.450, common air at a temperature of 60° being unity. A pound of air at 60° contains exactly 13.09 cubic feet; consequently, a pound of coal gas of a specific gravity 0.450 and at the same temperature contains:

$$\frac{13.09}{0.450} = 29.09 \text{ cubic feet.}$$

A cubic foot of such gas therefore weighs

$$\frac{1.00000}{29.09} = 0.0343 \text{ lbs.}$$

Taking the percentage of the constituents of coal gas as given above, we have the following for the weight in pounds of each in a cubic foot of gas:

Hydrogen	=	0.01501
Marsh Gas	=	0.01388
Carbonic Oxide	=	0.00203
Olefiant Gas	=	0.00225
Nitrogen	=	0.00036
Oxygen	=	0.00016
Carbonic Acid	=	0.00025
Aqueous Vapor	=	0.00034
		0.03428

Multiplying each of these weights by the caloric modulus for the corresponding element and we have the number of heat-units evolved in the combustion of that element, and the sum of these units will be the theoretical heat obtained by the combustion of a cubic foot of gas. The calculation referred to the Fahrenheit scale will stand as follows:

Hydrogen	$=34462\times0.01501\times1.8$	$=931.09$
Marsh Gas	$=13063\times0.01388\times1.8$	$=326.36$
Carbonic Oxide	$=3403\times0.00203\times1.8$	$=12.42$
Olefiant Gas	$=11857\times0.00225\times1.8$	$=48.00$
		1317.87 heat units.

The remaining constituents are non-combustible.

The caloric moduli for the different gases as here used were obtained by combustion in an atmosphere of pure oxygen, while combustion in practice will, of course, take place in common air. Comparing this calculated amount of heat with the observed amount, obtained by calorimetrical tests of illuminating gas of the candle power, and specific gravity above given, shows that a reduction of the calculated heat of forty per cent. is necessary to make calculation agree with experiment. This reduction is undoubtedly owing, largely, to imperfect combustion, in addition to difference of condition. Making such reduction we have

$1317.85 \times 0.6 = 790.7$ = approximate number of heat-units from the combustion of one cubic foot of sixteen candle power illuminating gas.

The mean of several tests was 705 heat-units per cubic foot of gas. We will take, for purposes of calculation, 750 heat units per cubic foot of gas.

Gas burners range from three to six cubic feet consumption per hour, and, where no special arrangements are made for removing the heat and products of combustion from the burners, a simple calculation will show the great influence upon the health of the occupants of an apartment exerted by this apparently insignificant source of contamination.

c. The production of carbonic acid from the lights. According to many careful experiments the carbonic acid from a sperm or paraffine candle equals 0.31 of a cubic foot per hour. Calculation based upon the average composition of coal gas shows that the combustion of a cubic foot of gas produces 0.43 of a cubic foot of carbonic acid. Such a calculation stands substantially as follows: Taking the average analysis of coal gas given above, we have by calculation from chemical equivalency the weight of carbon in a cubic foot of gas, and from this we derive the weight of oxygen necessary to convert that amount of carbon into carbonic acid. The weight of the carbon, plus the weight of the oxygen equals the weight of carbonic acid produced, and is found to be 343 grains. Density of carbonic acid gas is 1.5 at a temperature of 60°. A cubic foot weighs at that temperature then 801 grains, a cubic foot of air weighing 534 grains at that temperature. Dividing the weight of a cubic foot of carbonic acid by the weight of that gas from a cubic foot of coal gas, and we have 0.43 of a cubic foot as the amount of carbonic acid produced by the combustion of a cubic foot of coal gas.

It is evident then that a considerable amount of oxygen is required for the various processes of respiration and combustion going on in confined spaces, and that a further deterioration of the air is continually taking place by reason of the presence of the nitrogen previously mixed with the oxygen so removed; that is to say, oxygen the life-supporting agent in these various processes is continually being removed, while nitrogen the inert, useless element is left behind.

Air in its normal condition contains twenty-three per cent. by weight of oxygen, and seventy-seven per cent. of nitrogen. It will soon result, however, in any confined space where respiration and combustion are going on, that the amount of oxygen constantly decreases while the nitrogen and carbonic acid increase relatively to the amount of oxygen present.

A farther source of contamination is found in insufficient sewer connections, though this part of the subject has been so often discussed that it is unnecessary to consider it at length here.

WARMING.

According to General Morin's paper on warming and ventilation, a translation of which appears in the annual reports of the Smithsonian Institution for 1873–74, heating apparatus should be considered in three different respects:

1. In regard to economy of fuel.
2. In regard to effect on health.
3. In regard to comfort.

The nature of the service to be performed should always be considered in deciding upon the apparatus for fulfilling these considerations. For instance, where occupancy is at intervals, and then only for a short time, the first will decide the choice of method. In buildings occupied more or less continuously, the second consideration should have more weight, while in dwelling houses the second and third together should influence the choice.

The cheapest method of heating is by stoves, more than ninety per cent. of the heat being realized in this way. The first cost is also much less than by the other methods.

Comfortable and healthful heating is obtained by open fire-places, two types of which may be distinguished:

1. Ordinary fire-places.
2. Ventilating fire-places.

The ordinary fire-place takes its supply of air directly from the room and heats solely by radiation. Its heating effect does not exceed fourteen per cent. of the total amount of heat produced. It is healthful and comfortable but not at all economical.

Captain Galton, of the British army, invented a form of the ventilating fire-place which furnishes a very satisfactory solution of the problem of pleasant, healthful heating, combined with excellent ventilation. It consists of a grate with a flue leading therefrom as in the ordinary arrangement. The flue, however, is of a good conducting and radiating material, and passes up through an exterior flue, into which air is admitted from the outside at the back of the grate. This air is warmed by coming in contact with the interior flue, which gives it an upward tendency, and is withdrawn near the ceiling into the room in which the fire-place is located, or it may be conducted into the rooms of the second story. The air to support combustion is taken from the room warmed as in the previous plan. The heating effect realized by this method is thirty-five per cent. of the total heating power of the fuel.

Heating by steam or hot water is pleasant, and when in connection with proper ventilation exceedingly healthful. The heating effect realized is nearly ninety per cent.

Hot air, when properly regulated, is pleasant, and in connection with ventilation, healthful. Heating effect seventy-five per cent.

MECHANICS OF AIR.

At a temperature of 60°, 13.09 cubic feet of air weigh exactly one pound. A cubic foot at the same temperature weighs 534 grains. As already remarked, air in common with all other ponderable bodies obeys the laws of gravitation, and, because of the slight attraction between its particles, it is, like gases generally, extremely sensitive to changes of temperature or pressure.

According to Gay Lussac's law the volume of one and the same gas varies with the temperature.

With notations as follows:

V_0 = volume at 32° and barometer at thirty inches.

V = volume at the temperature t.

$a = 0.00204$ = coefficient of expansion, expresses the increase of volume for an increase in temperature of one degree Fahrenheit.

We have by formula

$$V = [1 + a(t-32)]V_0$$
$$= [1 + 0.00204(t-32)]V_0 \quad (1)$$

In case the air is farther heated to the temperature t_1 the corresponding volume becomes

$$V_1=[1+0.00204(t_1-32)]V_0 \quad (2)$$

the ratio of the volumes V and V_1 will be

$$\frac{V}{V_1}=\frac{1+0.00204\,(t-32)}{1+0.00204(t_1-32)} \quad (3)$$

According to Mariotte's law we have :

1. The density of one and the same quantity of air is proportional to its pressure.

2. The volumes of one and the same quantity of air are inversely as the pressures.

3. From 1 and 2 it follows that the densities are inversely as the volumes.

Let D=density at volume V and D_1= density at volume V_1 therefore

$$\frac{D}{D_1}=\frac{V_1}{V}=\frac{1+0.00204(t_1-32)}{1+0.00204(t-32)}. \quad (4)$$

Farther let P_0 equal pressure at 32°; P that at temperature t, and P_1 that at t_1 therefore

$$V=1+0.00204(t-32)\frac{P_0}{P}V_0. \quad (5)$$

and

$$V_1=1+0.00204(t_1-32)\frac{P_0}{P_1}V_0 \quad (6)$$

hence

$$\frac{V}{V_1}=\frac{1+0.00204(t-32)P_1}{1+0.00204(t_1-32)P}. \quad (7)$$

and

$$\frac{D}{D_1}=\frac{1+0.00204(t_1-32)P}{1+0.00204(t-32)P_1}. \quad (8)$$

therefore

$$V_1=V\left(\frac{1+0.00204(t_1-32)P}{1-0.00204(t-32)P_1}\right) \quad (9)$$

and

$$D_1=D\left(\frac{1+0.00204(t-32)P_1}{1+0.00204(t_1-32)P}\right) \quad (10)$$

Now suppose t=32° and P_1=P and D =0.081 in the last formula and we have,

$$D_1=\frac{0.081}{1+0.00204(t_1-32)}, \quad (11)$$

from which the density of the air can be found at any temperature, the pressure P being constant.

In case it is desired to calculate the density D at any pressure whatever, we may write in the above formula where b is any height of barometer and 29.92= height at temperature of 32° and D= 0.081—

$$\frac{P_1}{P}=\frac{b}{29.92}$$

and again taking t= 32° and D=0.081 we have

$$D_1=\frac{0.081}{1+0.00204(t_1-32)}\times\frac{b}{29.92} \quad (12)$$

According to the law of physics, known as the principle of Archimedes, *a body plunged into a fluid loses a part of its weight equal to the weight of fluid displaced.* This law may apply to three different cases :

1. The weight of the body may exceed the weight of the fluid displaced, or in other words, the mean density of the body may be greater than that of the fluid, in this case the body sinks.

2. The weight of the body may be less than that of the fluid displaced; in this case the body rises partly out of the fluid until the weight of the fluid displaced is equal to its own weight.

3. The weight of the body may be equal to the weight of the fluid displaced; in this case the two opposite forces being equal the body is in equilibrium and remains in any position in which it may be placed. (See Deschanel's Physics, Chapter IX, Principle of Archimedes).

We will consider the application of these laws in the case of common air, considering at first a single cubic foot.

It has been shown, that at a temperature of 32°, and under a pressure of 29.92 inches of mercury, every cubic foot of air weighs 0.081 pounds. While the temperature of the single cubic foot remains at that point we have, by case 3 above, a condition of equilibrium, and no movement or disturbance takes place, since an equivalent volume of the surrounding air has exactly the same weight.

Suppose, however, the temperature of the air is at 70° and that by coming in contact with some cold surface, as, for instance, a window pane, the temperature of the single cubic foot is reduced to 32°. The result of such reduction is by the preceding discussion threefold :

(1) Volume reduced,
(2) Density increased,

(3) Consequently it becomes heavier than surrounding air and tends to fall by a certain definite weight found as follows: Taking formula (11)

$$D=\frac{0.081}{1+0.00204(70-32)},$$

reducing we have

$$D=\frac{0.081}{1.077}=0.0752=\text{the weight of a cubic foot of air at } 70^{\circ}.$$

The volume under the new conditions may be found by formula (1).

The cubic foot of air in passing from the temperature of 70° to that of 32°, increases its weight by 0.081—0.0752=0.0058 pounds. Carrying this calculation into grains, we have weight at 70°=7000×0.0752=526.4 grains, weight at 32°=7000×0.081=567.0 grains, 567.0—526.4=40.6 grains=gain in weight of a cubic foot of air by reduction of temperature from 70° to 32°.

In cold weather, when the windows and outside walls are much colder than the general temperature inside, it is evident from the preceding that currents of air must be continually passing downward to the floor along such cold surfaces.

Suppose, farther, the air is at a temperature of 40° and by coming in contact with a heated surface is warmed to 98°. Density at 40°=0.0797 pounds. Density at 98°=0.0714 pounds. Or, a cubic foot in passing from temperature 40° to that of 98° loses weight by an amount equal to 0.0797—0.0714=0.0083 pounds=7000×0.0083=58.1 grains. Our particular cubic foot of air is then in the condition of case 2 of the principle of Archimedes. Its tendency is to rise and it continues doing so when unconfined, until a point is reached where the densities are again equal.

The temperature of the human body is 98° while 40° is about an average temperature of the air during the whole year. It is evident, therefore, that a ventilation of the body is continually taking place through the operation of natural causes. We should take advantage of the hint thus obtained from nature, and plan the ventilation of our dwellings and public buildings upon a more rational basis than at present seems to prevail.

It is evident, without special discussion, that movements similar to those just ascribed to the particular cubic foot of air are, in fact, constantly taking place throughout the whole atmosphere, not only out of doors but in as great a degree inside. The atmosphere, considered as a whole, can never be at rest, the slightest changes in temperature produce variations in volume, density and pressure, and these again are the causes of unending motion. In short, we may say the air is perpetually in unstable equilibrium.

The temperature at which inhabited spaces should be kept varies from 60° to 65°, depending to a considerable extent upon the nature of the operations carried on. It is certain, moreover, that the temperature can be kept lower without discomfort in well-ventilated apartments, than in those which are not well-ventilated. Upon this point the writer has recently made some investigations which may with propriety be introduced here.

The revised charter of the city of Rochester, of last year, created a Board of Health with more extended authority than that exercised by previous boards. Among other items the new board has authority to regulate the ventilation of public school-houses throughout the city. During the past winter the writer at the instance of said board made an extended examination of all the school-buildings to the number of twenty-five, and reported thereon at length. In the course of the examination it appeared necessary to ascertain exactly the temperature at which the several rooms were kept. In going through them and using a thermometer in each it very soon became apparent that there were extraordinary variations in the temperature. The school-rooms, numbering more than two-hundred, were then supplied with thermometers, and the teachers carefully instructed as to the manner of taking observations, &c. The observations were taken ten times a day for one week, the times of taking being as follows: At the beginning of school (9 A.M.); at end of first hour, (10 A.M.); before morning intermission; (10.45 A.M.); after morning intermission, (11 A.M.); at end of morning session, (12 M.); at beginning afternoon session (1.30 P.M.); end first hour (2.30 P.M.); before inter-

mission, (2.45 P. M.); after intermission, (3 P.M.); end of afternoon session (4. P.M.).

These observations in connection with a careful consideration of the facilities for ventilation furnished matter of the highest interest. They led, indeed, to the inevitable conclusion that in schoolhouses where the ventilation is thorough, pupils and teachers are perfectly comfortable at a temperature of 60°–62°, and that the temperatures invariably increase as perfection of ventilation decreases. So true is this that the following laws are fairly deducible, which so far as an nquiry into the ventilation of schoolhouses is concerned, may be denominated the laws of temperatures.

1. In rooms having but one outside exposure the temperature is uniformly higher than in those having two or more outside exposures, other conditions being the same.

2. Where the communication is direct by means of roomy halls between lower and upper floors, the temperature ranges higher on upper floors.

3. Of two rooms having equal exposure and equal heating and ventilating facilities, the one containing the greater number of pupils will show the higher average temperature.

4. There is a relation between inside temperature, outside temperature and outside humidity, which relation appears to be expressed by saying that inside temperature varies directly as outside humidity and inversely as outside temperature.

The writer cannot at present vouch for the entire correctness of this last law; the observations were somewhat conflicting, though the preponderance is decidedly in favor of the law. It stands at present, however, as an inference rather than a positive induction.

The temperatures show invariably an increase from beginning of school in morning (9 A.M.) to end of first hour (10 A.M.). They also show a corresponding increase from beginning of school in afternoon (1.30 P.M.) to end of first hour (2.30 P.M.). In buildings of no ventilation, and where, during the winter, flushing with fresh air is resorted to by opening windows at intermissions, the thermometers were naturally lower after intermission than before; while in those where flushing is not resorted to, the temperatures were substantially the same. The maximums were usually obtained before intermission, and at end of session. Observations on humidity were also taken in several of the buildings, and it was found that a great increase of moisture in excess of that outside took place in poorly-ventilated rooms, pointing to the conclusion that excess of temperature is accompanied by excess of moisture, and to the farther consideration that excess of temperature, and excess of moisture are certain indications of defective ventilation.

Judging from the showing of the temperature records a great difference of opinion exists as to the proper temperature at which a room should be kept. The rooms are undoubtedly kept at the point of comfort and that point varies as perfection of ventilation varies. For instance, the lowest temperatures occur in well-ventilated rooms, while the highest occur in those poorly-ventilated. In rooms where the temperature ranges from 62° to 66° the question was asked of the teachers in at least twenty places, "Do you find the room too warm when much above 66°? The reply usually was, "We do." In the case of rooms with an average temperature of 68°–71° the form of question was, "Do you find the room too cold when much below 68°? The reply was in nearly every case, "We do." The general principle, therefore is, in imperfect ventilation the exhalations from the body and breath produce an excess of moisture in the air, and, consequently, an increase of relative humidity. Our sensations of heat and cold depend (within limits) as much upon the amount of moisture present in the air as upon the actual temperature. A high relative humidity always produces chilliness. We instinctively, therefore, increase the heat as the moisture increases. Moreover the air's capacity for moisture increases as the temperature. In illy-ventilated apartments, the two go together, increase of moisture leading to increase of heat, increase of heat leading to increase of moisture. Aside from the mere waste of heat resulting from this condition of affairs, there is matter of more serious import to be considered. Where bad ventilation exists the windows and doors are thrown

wide open for flushing at intermissions and noontime. These results are immediate depression of temperature, in some cases sufficient to fall below the dew point, hence moisture is actually deposited as dew about the room, and hence colds and sickness of teachers and pupils generally. The same thing will occur not only in school rooms, but in any illy-ventilated apartment where a large number of people being congregated, windows are suddenly opened for the admission of fresh air.

The question of humidities is somewhat complicated. It will be eminently proper, therefore, to consider briefly the principles involved; and in making such considerations Deschanel's Physics will be drawn upon. With this general acknowledgement such use is made of the excellent work in question as may be necessary to the proper elucidation of the subject in hand.

The condition of the air as regards moisture involves two distinct elements: (1) the amount of vapor present in the air, and (2) the ratio of this to the amount which would saturate the air at the actual temperature. There are two important laws bearing upon the subject:

1. The weight of vapor which will enter a given space is the same whether this space is empty or filled with a gas.

2. When a gas is saturated with a vapor the actual tension of the mixture is the sum of the tensions due to the gas and the vapor separately; that is to say, it is equal to the tension which the gas would exert if it alone occupied the whole space plus the maximum tension of vapor for the temperature of the mixture.

The word *tension* means in this connection the force acting to produce expansion, as opposed to pressure, or the force tending to produce compression, or we may say the tension decreases the density of a gas while pressure increases that element.

Relative humidity is defined as the weight of aqueous vapor in a given volume of air, expressed as a percentage of the weight of vapor at saturation which would occupy the same volume at the actual temperature.

When air containing water in form of vapor is gradually cooled at constant pressure, its capacity for vapor gradually decreases until the point of saturation is reached. Any farther reduction of temperature is accompanied by a deposit of moisture. This point of deposit is called the dew point, the relation of which to the actual temperature is shown by the humidity tables below.

There is a popular idea that air loses its moisture by heating. In fact, since moisture exists in the air, so far as known in the form of vapor and not mechanically suspended, the only way the air can lose moisture is by reduction of temperature to the dew point, and by consequent deposition in the form of dew. The effect of heating air is to increase the capacity for moisture, accompanied by decrease of relative humidity, an apparent dryness may thus be attained, although the absolute humidity remains the same as previous to the heating.

We will now consider the tables given below.

Table No. 1 is a general exhibit of the condition of ventilation at school-house No. 5, with one exception the best ventilated public building in the city of Rochester. This building is warmed by heated air, from three large radiating furnaces placed in the basement. The heating apparatus has, however, one serious defect in not permitting an adequate supply of fresh air, owing to the supply conduit being too small. The result is that the currents of air on reaching the rooms show a temperature much higher than they should. Ventilation is by means of open fire-places in each room in which a fire is kept burning only during shool hours. The flues from the fire-places are of ample capacity to carry away the impure air, and, however crowded the rooms may be, the air is always fresh and pleasant.

Table No. 2 is a similar exhibit for school-house No. 13, one of the worst ventilated buildings in the city. This building is warmed throughout by stoves.

Table No. 3 is a similar exhibit for school No. 9, a complete discussion of which will be found afterwards.

Table No. 4 is a humidity table for No. 5, while table No. 5 is a similar exhibit for school No. 13.

The tables are generally self explanatory, though one or two points may be specially noticed.

No. 1.—Table for No. 5.

No. of grade.	No. boys belonging.	No. girls belonging.	Total No. pupils in room.	No. sittings in room.	Floor area in square feet.	Ratio of sittings to floor area square feet.	Volume in cubic feet.	Ratio of sittings to volume cubic feet.	Deficiency of air space for each pupil in cubic feet.	Amount of air to be supplied per hour in cubic ft.	No. changes of air required per hour.	Estimated capacity of exhaust per hour in cubic ft.	Deficiency of exhaust per hour in cubic feet.	No. outside exposures.	No. windows.	Temperature Fahrenheit.	Maximum.	Minimum.	Range.
9th	40	29	69	55	704	12.80	9559	174	0	46750	4.9	50000	0	2	6	65.27	70°	60°	10°
8th	25	24	49	55	749	13.62	10150	185	0	57750	5.6	60000	0	2	6	64.72	70	60	10
7th	19	18	37	53	749	14.12	10152	191	4	62010	6.1	63000	0	2	6	62.17	68	54	14
6th	28	28	56	47	704	14.99	8893	189	36	63450	7.1	65000	0	2	6	65.02	68	62	6
5th	25	31	56	49	704	14.28	8891	181	59	70560	7.9	72000	0	2	6	66.56	70	62	8
4th	21	28	49	47	749	15 94	9470	201	62	74260	7.9	75000	0	2	6	67.06	72	60	12
3d	19	31	50	47	749	15.94	9470	201	89	81780	8.6	83000	0	2	6	67.06	71	58	13
2d	16	17	33	36	704	19.55	11012	306	2	66600	6.0	45000	21600	2	6	66.39	72	58	14
1st	15	31	46	42	749	17.83	11703	279	54	84000	7.1	45000	39000	2	6	67.12	72	57	15

No. 2.—Table for No. 13.

No. of grade.	No. boys belonging.	No. girls belonging.	Total No. pupils in room.	No. sittings in room.	Floor area in square feet.	Ratio of sittings to floor area square feet.	Volume in cubic feet.	Ratio of sittings to volume cubic feet.	Deficiency of air space for each pupil in cubic feet.	Amount of air to be supplied per hour in cubic ft.	No. changes of air required per hour.	Estimated capacity of exhaust per hour in cubic ft.	Deficiency of exhaust per hour in cubic feet.	No. outside exposures.	No. windows.	Temperature Fahrenheit.	°	°	°
9th D	24	17	41	48	343	7.35	3386	70	72	40800	12.1	0	40800	1	2				
9th C	18	15	33	44	350	7.70	3456	79	63	37400	10.8	0	37400	1	2				
9th B	18	20	38	40	313	7.82	3240	81	61	34000	10.5	0	34000	1	2				
9th A	23	12	35	42	389	9.26	4292	102	40	35700	8.3	0	35700	1	4				
8th B	24	30	54	56	781	13.95	9476	169	6	58800	6.2	0	58800	2	4	68.16	73	62	11
8th A	20	36	56	64	775	11.95	9394	147	28	67200	7.1	0	67200	1	2	67.91	78	62	16
7th	36	33	69	80	761	9.51	9200	115	80	93600	10.2	0	93600	1	2	68 22	72	61	11
6th B	20	18	38	58	775	13.36	9395	162	63	78300	8.3	21000	57300	1	2	68.64	75	62	13
6th A	23	18	41	56	794	14.18	9612	172	53	75600	7.8	21000	54600	2	5	65.53	72	60	12
5th	29	21	50	50	754	15.08	11818	236	4	72000	6.1	14000	58000	2	5	70.19	78	62	16
4th	16	25	41	46	787	17.11	12292	265	0	72680	5.9	21000	51680	1	2	70.05	76	56	20
3d	29	23	52	53	772	14.56	11975	226	64	92220	7.7	14000	78220	1	2	70.41	75	63	12
2d	18	10	28	42	779	18.55	12110	288	20	77700	6.4	0	77700	1	2	70.35	76	62	14
1st	11	21	32	36	786	21.83	12369	343	0	72000	5.8	0	72000	2	4	70.84	76	64	12

The column of "amount of air to be supplied per hour," is obtained by multiplying the number of sittings in each room by the corresponding factor from the table of "amount of air to be supplied for each pupil in the different grades" (Table No. 7). This factor was obtained as follows: The empirical assumption was made that the pupils in first grades require 2,000 cubic feet of air per hour, and that in the lower grades they require an amount proportional to their ages. The column of "amount of air space for each pupil" of this table was obtained in this way: It is well established that where change of

No. 3.—Table for No. 9.

No. of grade.	No. boys belonging.	No. girls belonging.	Total No. pupils in room.	No. sittings in room.	Floor area in square feet.	Ratio of sittings to floor area square feet.	Volume in cubic feet.	Ratio of sittings to volume cubic feet.	Deficiency of air space for each pupil in cubic feet.	Amount of air to be supplied per hour in cubic ft.	No. changes of air required per hour.	Estimated capacity of exhaust per hour in cubic ft.	Deficiency of exhaust per hour in cubic feet.	No. outside exposures.	No. windows.	Temperature Fahrenheit.	Maximum.	Minimum.	Range.
9th D	32	25	57	56	416	7.43	5369	96	46	47600	8.9	48000	0	2	4	65.94	75°	56°	19
9th C	28	26	54	56	435	7.77	5442	97	45	47600	8.7	48000	0	1	2	67.51	75	57	18
9th B	29	17	46	56	427	7.62	5408	96	46	47600	8.9	48000	0	2	4	67.39	72	58	14
9th A	39	13	52	56	435	7.77	5442	97	45	47600	8.7	48000	0	1	2	68.64	79	57	22
8th	24	21	45	56	433	7.73	5417	97	78	58800	10.8	60000	0	1	2	67.74	76	58	18
7th	19	27	46	56	439	7.84	5492	98	97	65520	11.9	67000	0	1	2	67.93	78	56	22
7th&6th	32	24	46	64	437	13.05	9878	154	56	80640	8.2	82000	0	1	2	68.59	75	56	19
6th	19	22	41	41	425	10.36	6208	152	73	55350	8.9	57000	0	2	4	68.17	75	60	15
5th	15	17	32	41	435	10.61	6292	153	87	59000	9.4	60000	0	1	2	68.51	72	60	12
4th B	12	15	27	41	435	10.61	6292	153	110	64780	10.2	66000	0	1	2	66.99	70	58	12
4th A	12	15	27	40	427	10.67	6255	156	107	63200	10.1	65000	0	2	4	69.11	74	60	14
3d	11	14	25	40	433	10.83	6273	156	134	69600	11.1	71000	0	1	2	69.00	75	60	15
2d	13	13	26	36	433	12.03	6273	174	134	66600	10.6	68000	0	1	2	67.68	71	59	12
1st	15	5	20	32	865	27.03	13361	417	0	64000	4.8	65000	0	1	2	68.35	74	59	15

No. 4.—Humidity Table for No. 5.

Date of observation.	Time of observation.	Grade in which observations were taken.	Barometer.	Actual temperature.	Temperature of dew point.	Force of vapor in inches of mercury.	Amount of vapor present in a cubic foot of air in grains.	Amount of vapor required to complete saturation at actual temperature in grains.	Relative Humidity.
Tuesday, March 6, 1877.	From 2 P. M. to 4 P.M.	9th		61°	42.3°	0.286	3.21	2.85	53.0
		8th		64	41.9	0.282	3.15	3.50	47.3
		7th		65	42.6	0.289	3.22	3.65	46.9
		5th		68	48.8	0.358	3.96	3.57	52.6
		4th		68	45.6	0.321	3.54	3.99	47.0
		2d		65	44.2	0.306	3.39	3.48	49.3
		1st		68	45.6	0.321	3.54	3.99	47.0

Outside Humidity for same Date.

11.58A.M		30.210	20	5.5	0.060	0.71	0 59	55.6
2.00 P.M.		30.128	22	3.5	0.069	0.80	0.61	58.4
4.33 P.M.		30.007	25	9.0	0.067	0.68	0.68	50.0

air, in average apartments, occurs at a greater rate than six times an hour, currents and unpleasant drafts are produced, which are likely to seriously affect the occupants. The limit of space for a single person will, therefore, be the amount of air to be supplied to each person divided by six. In this way the column in question was obtained.

In tables 1, 2 and 3 the column of "no changes of air required per hour" is the number of changes required per hour to

No. 5.—Humidity Table for No. 13.

Date of observation.	Time of observation.	Grade in which observations were taken.	Barometer.	Actual temperature.	Temperature of dew point.	Force of vapor in inches of mercury.	Amount of vapor present in a cubic foot of air in grains.	Amount of vapor required to complete saturation at actual temperature in grains.	Relative Humidity.
Tuesday, March 8, 1877.	From 1.30 P. M. to 4.00 P. M.	8th B		63°	46.0	0.326	3.63	2.82	56.3
		8th A		65	50.6	0.381	4.23	2.64	61.6
		7th		70	55.0	0.442	4.87	3.13	60.9
		6th B		70	55.0	0.442	4.87	3.13	60.9
		6th A		67	52.6	0.408	4.52	2.78	61.9
		5th		71	54.5	0.435	4.78	3.47	57.9
		4th		71	53.0	0.414	4.54	3.71	55.0
		3d		71	53.0	0.414	4.54	3.71	55.0
		2d		70	52.0	0.400	4.40	3.60	55.0
		1st		70	52.0	0.400	4.40	3.60	55.0
Outside Humidity for same Date.									
	2.00 P.M.		29.569	42°	37.6	0.243	2.83	0.47	85.8
	4.33 P.M.		29.491	43	38.6	0.252	2.93	0.48	85.9

Table No. 6 *is an exhibit of the mean daily temperature in several of the school buildings in the City of Rochester for each of the days on which observations were taken. It shows unmistakably that the same cause has modified the result throughout the whole series. For instance, the range is higher on Tuesday than on Monday, lower on Wednesday, higher than Wednesday on Thursday, and generally still higher on Friday, though there are exceptions to this as will be seen by inspection.*

No. 6.

No. of school.	Monday, Feb. 26, 1877.	Tuesday, Feb. 27, 1877.	Wednesday, Feb. 28, 1877.	Thursday, March 1, 1877.	Friday, March 2, 1877.
2	64.12	65.46	64.48	65.58	66 57
3	66.35	66.83	65.62	66.46	67.16
4	65.87	66.09	65.84	66.89	66.01
5	65.01	66.06	65.54	66.51	65.54
6	67.52	68.20	67.82	67.74	67.77
8	61.25	65.05	65.05	66.60	66.17
9	68.13	68.26	67.39	67.73	68.31
11	68.26	69.75	65.56	66.00	66 44
12	66.32	66.80	66.00	66.20	67.73
13	68.21	68.62	66.99	70.16	69.06
16	69.75	71.05	70.44	71.71	71.64
17	68.26	68.40	68.24	68.31	70.38
18	67.02	67.34	67.29	68.61	68.00
19	67.30	66.71	65.69	66.12	65.66
20	65.43	67.33	65.66	67.27	68.22
Means	66.58	67.46	66.50	67.45	67.64

No. 7.

Table of amount of air to be supplied for each pupil in the different grades.

Grade.	Average age in years.	Amount of air for each pupil per hour in cubic feet.	Amount of air space for each pupil in cubic feet.
9th	6.26	850	142
8th	7.76	1050	175
7th	8.66	1170	195
6th	9.52	1350	225
5th	10.69	1440	240
4th	11.72	1580	263
3d	12.90	1740	290
2d	13.69	1850	308
1st	14.88	2000	333

keep within the standard of ventilation here assumed.

The column of "deficiency of air space for each pupil" is a comparison between the column of ratio of sittings to volume and the "amount of air space for each pupil," and shows what addition to the volume for each sitting would be necessary to bring the room within the limits of scientific ventilation.

The temperatures in tables 1–3 are the averages of all observations.

The necessary calculations of humidities are from Glaisher's tables, while the

outside humidity record is from the signal service office at Rochester.

According to General Morin, the amount of air to be changed every hour, in order to keep within the limits of good ventilation is as follows:

	Cubic feet.	
Hospitals	2.119	3.709
Prisons		1.776
Workshops	2.119	3.532
Barracks	1.059	1.776
Theatres	1.413	1.776
Lecture and assembly rooms, &c.	1.059	2.119
Schools	.424	1.059

Having decided how much air shall be supplied per unit of time, the next question is how shall the supply be furnished? This introduces us to our subject proper, *The Mechanics of Ventilation.*

Two methods of producing the result may be distinguished, the *vacuum* and the *plenum.* We confine ourselves entirely to a consideration of the former, as being more in accordance with the natural order of affairs, and as having been demonstrated by experience to give the better results.

The vacuum method requires that every apartment or building shall have, suitably connected with it, a vertical shaft, in which an exhaust draft is constantly maintained. The problem in hand, then, is the determination of the dimensions of such a shaft and its contingent connections, together with such special considerations as shall make the method applicable to any case whatever.

The point of withdrawal will be as near the source of contamination as convenient, that is, at or near the floor. They should never be in the floor itself, as dust, etc., will collect ultimately causing unpleasant stoppages. The points of ingress should, on the contrary, be as far away from the contamination as possible, or at the ceiling. It may be taken as a general law that whatever the method of heating resorted to, foul air should be drawn out of a room by creating a vacuum at some point in the side walls near the floor, while fresh air should be introduced at or near the ceiling.

We have seen that heating a limited amount of air to a temperature higher than the surrounding atmosphere causes the heated portion to ascend. This then offers in many cases a cheap and efficient means of ventilation, by simply utilizing the large amount of heat now wasted through the chimneys. It should be noticed in passing that although there is a great waste of heat at present through the chimneys, it seems certain that improved ideas of chimney building will soon in a great degree correct that evil. In the meantime, the ventilation of structures already erected may be improved by taking advantage of the method here indicated.

The formula expressing the conditions of movement of heated air in a vertical flue is

$$V=E\sqrt{2g\frac{ha\,(t'-t)}{1+at}} \quad . \quad (13)$$

where the notation is

V=velocity of ascent of air current per second.

E=a numerical coefficient, constant for each shaft, and expressing the reduction of the theoretical velocity due to friction, eddies, &c.

g=the acceleration of gravity numerically equal to 32.2 lineal feet.

h=the height of the chimney or flue, and the consequent height of the confined column of air.

a=coefficient of expansion of air =0.00204 as previously given.

t'=temperature within the chimney, and

t=temperature of outside air.

This formula is merely an expression in extended form of Torricelli's theorem, which asserts that the velocity of any freely falling body, is equal to the square root of twice the acceleration of gravity, into the height through which the body falls. Expressing this more elegantly as an equation, and we have

$$V=\sqrt{2g\,h'} \quad . \quad . \quad . \quad (14)$$

where g has the value just given and h' is the height through which the body falls.

In formula (13)

$$\frac{ha\,(t'-t)}{1+at}=h\frac{1+at'}{1+at}-h=h' \quad . \quad (15)$$

We have seen by previous discussion that when air is heated at constant pressure the volume increases. Now in case the heating takes place in a flue

open only at the ends, it is evident the expansion must cause an increase in the length of the confined column. Such increase is expressed by h'. Moreover the difference between the original length of the column at temperature t, and the new length at temperature t', is equal to the height through which a volume of outside air equal to the original volume within the flue has fallen, to produce the upward movement within the flue; h' is, therefore, the head of fluid producing pressure, and may be calculated by the general formula for head of fluids flowing through pipes, &c., following:

$$h'=1+\xi_1+\xi_0+\left(\xi\frac{l(a+b)}{2ab}\right)\frac{V^2}{2g} \quad (16)$$

This formula means, that having given the cross-section of a flue, pipe or conduit, and the amount of air to pass through per unit of time, which of course determines the velocity v, per unit of time, the height of column of air necessary to produce the velocity v through the given cross-section, is equal to h' as expressed by the above formula, the notation of which is as follows:

h' has the value previously given.

ξ_1, ξ_0 and ξ are coefficients of resistance of elbows, efflux and influx and friction respectively.

l=length of pipe.

a and b=dimensions of cross-sections.

v=velocity of current per unit of time, and

g=acceleration of gravity as previously given.

In case the flue is cylindrical the expression

$$\left(\xi\frac{l(a+b)}{2ab}\right) \text{ will become } \xi\frac{l}{d}$$

General formula for velocity is

$$V=\frac{Q}{F} \quad . \quad . \quad . \quad . \quad (17)$$

where Q=quantity discharged per unit of time, and F=sectional area of duct.

Formula (13) enables us to say what the conditions of discharge are in a vertical flue. It depends upon :

1. The height h; that is the power of the flue, or its velocity of discharge increases as the square root of the height increases, the difference of exterior and interior temperatures remaining constant.

2. The square root of the difference of t' and t; that is with a given height of flue the velocity of discharge will increase as the difference between interior and exterior temperatures increases.

3. The resistance to ascent of currents of air by friction and eddies; that is the velocity of discharge, will increase as the smoothness, straightness and adaptation of the cross-section of the flue to the amount of air to be disposed of increases.

It follows from these conditions that having the height, sectional area and general arrangement of any chimney or flue, the quantity of air or products of combustion discharged per unit of time will always be the same, provided the excess of temperature inside the chimney over that of the outside air remains constant.

We will now make a numerical application of the principles here set forth; the example selected for such purpose being school building No. 9, in the city of Rochester.

The object in taking this particular case is that a system of ventilation by the vacuum method was recently constructed at that building. Owing, however, to neglect of the principles here set forth, hardly moderate success was attained. The discussion will therefore include not only the numerical application to that building, but also critical notes on the ventilating works as they were actually carried out.

By referring to the table No. 9, given above, it will be seen that the building contains fourteen rooms, and that the amount of air to be renewed per hour is 837890 cubic feet. The volume of halls is in round numbers 7000 cubic feet. Adding for change of air about twice each hour in halls, and we may take the total amount to be renewed per hour at 850000 cubic feet. The building is two stories high, having seven rooms to each floor, and is L shaped, fronting upon two streets. In the inside angle of the L a shaft fifty-five feet in height was erected, straight inside, with sectional area top and bottom equal to 26.62 square feet. From the foot of the shaft large ducts were carried to either extremity of the L.

The main ducts were ($20'' \times 44''$); secondary ones ($16'' \times 36''$); tertiary ones ($16'' \times 22''$); and so on, decreasing as distance from main shaft increased. Into these horizontal ducts were let vertical ones which connected with the rooms by openings in floor, over which iron registers were set. The number of opening and corresponding ducts from each room is six. Sectional area of each of these small ducts ($6'' \times 8''$) $\times 48$ square inches. The elbows are in every case square. The number of small ducts leading to halls is 13, and since there are 6 to each room, and 14 rooms the total number of exhaust openings is found to be 97. We will assume for present purposes that the openings have equal capacities, though the variations of resistance because of elbows and difference of length of small ducts leading to main ducts in cellar give them in fact, varying capacities. On this assumption each duct will take away a quantity per hour equal to

$$\frac{850000}{97} = 8763 \text{ cubic feet.}$$

The amount removed per second by a single duct will be

$$\frac{8763}{3600} = 2.434 \text{ cubic feet.}$$

According to formula (17)

$$V = \frac{2.434 \times 1728}{6 \times 8 + 12} = 7.3 \text{ lineal feet.}$$

In fact the free area of opening is not more than two-thirds of the sectional area of pipe, owing to the register set over the opening. Or the velocity through register will be

$$\frac{4206}{32 \times 12} = 10.9 \text{ lineal feet.}$$

THE POSITION AND PROSPECTS OF THE BRIISH IRON TRADE.

From "The Engineer."

NEXT to the extent and duration, accessibility and cheapness, of our coal supplies, there is no subject of greater importance to the industrial interests of Great Britain than the available resources existing within our shores for the production of cheap and useful iron. One committee after another has sat upon the coal question, which has now been more or less exhausted in every phase it is capable of presenting. But of our mineralogical resources, so far as the manufacture of iron is concerned, we have but the most scanty available information; and that, too, scattered over such a wide area, and presented in such fragmentary and sectional contributions, that it can only be read and known by a few. It is almost superfluous to point out that not only are the progress and prospects of the iron trade of cardinal importance for its own sake, as the most extensive, utilitarian, and valuable of all our metallurgical industries, which is, to a very large extent, the foundation and source of all the collateral avenues of wealth and prosperity that have caused England to be distinguished as "the workshop of the world;" but also, because there is no other influence that so nearly and so directly affects the still more vital consideration of the future of our coal supplies, seeing that not less than forty million tons of coal, or nearly one-third of the total quantity raised in the United Kingdom, are employed, one way and another, in the manufacture of iron.

There never was a time when the iron trade suffered more acutely from adverse influences than it does now. Into the whole *rationale* of the process whereby that suffering has been brought about we shall not attempt to enter; but it cannot be otherwise than a matter of deep and anxious concern to the trading and commercial interests of the country at large, to examine certain phenomena which seem on the first blush to justify the conclusion that, for the first time in its history, the iron trade of England has entered on a retrograde career, and has come very near to if it has not actually gone beyond its legitimate development.

Let it be clearly understood that this is put forward simply as an appearance and not as a fact; but the appearance is sufficiently startling, and is environed with so many attendant circumstances not heretofore examined, that we need make no apology for formulating and analysing the actual facts.

It may be well to clear the way by explaining that the iron trade is almost entirely a product of the last half century. In 1740, only 17,000 tons of pig iron were produced in the United Kingdom, from fifty-nine blast furnaces—a production scarcely equal to that of a single fair-sized blast furnace at the present time. In 1796, the annual production of pig iron was 125,000 tons, the number of blast furnaces having, meanwhile, advanced to 130. Thirty-four years later the production had attained the extent of 678,417 tons, the trade, according to the evidence of Sir John Guest, having remained stationary between 1823 and 1831. But from the latter year it began to make rapid strides, until Mushet found a total make of 1,248,781 tons in 1839, the number of furnaces then in blast being 396. In his well-known work on the "Progress of the Nation" Mr. G. R. Porter, F.R.S., returned the make of pig iron in 1840 at 1,396,400 tons, and he calculated the quantity of coal consumed in the operation of smelting that production at 4,877,000 tons. Adding to this, 200,000 tons of coal used in converting the crude iron into wrought iron, it will be found that the total bulk of coal consumed in our iron manufacture thirty-six years ago was only 6,877,000 tons, as compared with about forty million tons at the present time. Steady progress continued to be made in the pig iron trade until 1855, when the total production was returned at 3,218,154 tons, the make having advanced rather more than two million tons in fifteen years. During that period a strong impulse had been given to the development of trade from two remarkable events—the first being the introduction of the hot blast, which reduced the consumption of coal used for smelting a ton of pig iron by nearly two-thirds; and the second, the commercial discovery of the Cleveland ironstone in 1850, which brought about the development of a much cheaper and more extensive field of iron ore than any formerly known to exist. Up to 1858 the growth of the pig iron trade had proceeded almost without a check. Commercial depression, however, caused the make of that year to fall about two hundred thousand tons below that of the previous year, while the estimated value of the 3,456,064 tons then produced fell to £8,640,160, as compared with £9,148,617, the value of the produce of 1857. During that *annus mirabilis* of commercial disaster, 1866, the accretion of growth was again hindered, the make having fallen from 4,819,254 to 4,523,897, and the estimated value from £12,048,133 to £11,309,742. But the dawn of another season of comparative prosperity speedily restored the old order of things, and during the next five years, the expansion of our metallurgical industry was great beyond all precedent, the increase between 1866 and 1871 having been over two million tons per annum, or an increment more than double the whole make of the country in the year 1830. The main increase was between 1870 and 1871, when the make of pig iron rose from 5,963,515 tons to 6,627,179 tons; the quantity of pig iron exported from 753,339 tons to 1,057,458 tons, and the total quantity of coal used in the manufacture of both crude and finished iron from 35,344,634 tons to 38,540,102 tons.

It is now that we come to face the strange, and somewhat inexplicable fact, that since 1871 there has been a diminution both in our production of pig iron and in the quantity of iron ore raised in the United Kingdom. Such a result is not only completely at variance with the experience of the past, but belies both the calculations and expectations of every authority on the subject of our industrial resources. To the large and rapidly accumulating demands made by the iron trade was attributed the origin of the coal famine, and the Royal Commission appointed to inquire into the operation of that event found that the order of things had been: first, the rise in the price of iron, then the rise in the price of coal, and, finally, the increase in the rate of wages. Every one who took the trouble to inquire into the causation of events concluded that the future development of the coal trade would be largely determined by the demand for

iron-making purposes; and yet we find the altogether bewildering truth to be that concurrently with a considerable falling off in the consumption for iron making, there has been a very large increase in the aggregate production of coal. The total quantity of pig iron made in the United Kingdom in 1875 was only 6,365,462 tons, or in round figures, about two hundred and fifty thousand tons less than in 1871. The total quantity of coal used in the manufacture of pig iron in 1875 was 15,645,774 tons, as compared with 19,881,537 tons in 1871, being, in round figures, a decrease of four million tons; but while this decrease took place in the consumption of coal for iron-making purposes, we find, not as might have been expected, a corresponding falling off in the production of coal, but an increase of production to the extent of about fifteen million tons! Hence, therefore, the fact is clearly established that, although the iron trade as a whole—including both its crude and its finished products—absorbs nearly a full third of all the coal raised in the country, the development of our coal supplies is not only not directly contingent upon the demand for iron-making, but follows certain lines with which that demand has little or nothing to do, and may henceforth be expected to proceed without that regard to the requirements of our chief manufacturing industry on which the Royal Commissioners and other authorities have laid so much stress.

The origin and operation of the apparent decadence of the iron trade of Great Britain form a still more important and perplexing source of inquiry. In 1871 we raised in the United Kingdom 16,334,888 tons of iron ore. In 1875, the total quantity of iron ore raised was only 15,821,060 tons. In the former year, however, we imported only 324,043 tons of iron ore, as compared with 458,693 tons in the latter; and the burnt or purple ore imported within the same period advanced from 200,000 to 280,000 tons. The increase in the latter items is too trifling, perhaps, to justify the conclusion that our supplies of native ore are becoming exhausted, and that we will henceforth be more and more dependent upon foreign supplies; but this is an inference fairly warranted by the greatly increased demand for good hematites suitable for the steel manufacture, of which in this country we have such a limited supply that we are compelled year by year to increase our imports from Spain, Algeria, and elsewhere. The following figures exhibit the relative increase or decrease of iron ore, produced in the several districts yielding that mineral in the United Kingdom, between the years 1871 and 1875, the latter year being the most recent for which authentic returns are as yet available:

Name of District.	1871.	1875.
	Tons.	Tons.
Cornwall	21,947	11,403
Devonshire	14,124	10,594
Somersetshire	32,883	45,165
Nottinghamshire	—	11,750
Gloucestershire	207,598	111,825
Wiltshire	159,894	87,152
Oxfordshire	28,330	34,568
Northamptonshire	779,314	1,085,898
Lincolnshire	290,673	573,366
Shropshire	415,972	240,568
Warwickshire	34,075	97,456
Staffordshire, North	1,513,080	939,023
Do. South	705,665	715,451
Cheshire	—	1,500
Derbyshire	492,973	218,132
Lancashire	931,048	834,484
Cumberland	1,302,703	1,147,968
Yorkshire, North Riding	4,581,901	6,121,794
Do. West Riding	407,997	353,582
No'humberl'd & Durham	285,297	60,515
North Wales	51,887	42,184
So'th Wales & Monmouth	969,714	495,840
Isle of Man	75	—
Scotland	3,000,000	2,452,235
Ireland	107,734	128,602
Totals	16,334,888	15,821,060

The reader will find, if he takes very slight trouble in the analysis of the above statistics, that most of the older districts are going to the wall, having either become nearly exhausted, or furnishing their produce at such a heavy cost as to render it commercially impossible to proceed with their further development; and this fact is abundantly attested by the difference in the value of the ores raised during the years above named. We find that although there has been a decrease of 513,000 tons in the quantity of iron ore produced in 1875 as compared with 1871, the difference in value is not less than £1,695,000. Such a circumstance, moreover, is all the more remark-

able when we bear in mind that the average price of pig iron in 1875 was considerably above that of 1871. The mean price of Cleveland pig iron during 1871 was £2 9*s*. 6d. per ton, whilst in 1875 it was £3 per ton; and Scotch pig iron averaged £3 2*s*. 2d. in the former year as compared with £3 4*s*. in the latter. Broadly stated, the above table brings out the fact that it has become necessary for the English ironmaster to discard the expensive ores of such counties as Cornwall, Devonshire, Gloucestershire, Wiltshire, Shropshire, and Derbyshire, in favor of the less expensive and coarser ores of Cleveland, Northampton, and Lincolnshire; and these three new districts are consequently elbowing the older districts out of the race of competition and usurping their places. Between 1871 and 1875 two new iron ore-producing districts—Notts and Cheshire—have come into the field, but neither of them has as yet assumed any real importance; and to all practical intents the ironmasters of England must look in the future towards Cleveland, whose supplies of ore, illimitable in extent, are now being worked out at the rate of about seven million tons per annum; Lincolnshire, where the iron ore is more cheaply worked than any other part of the country, although the absence of an immediately contiguous coal-field places the smelter at a disadvantage; and Northamptonshire, whence large supplies of ore are now sent to older districts, whose resources are more scanty and precarious. It may be accepted as an industrial axiom of the iron trade that when a district no longer depends upon itself alone for raw material it must cease to advance if it does not absolutely tend towards decay. And this is precisely the position in which Wales and Staffordshire are now placed. The production of iron ore in South Wales has been diminished one-half between 1871 and 1875, and in North Staffordshire has gone more than half a million tons to the bad as between the two dates. Scotland, another of our most venerable centres of metallurgical industry, has been reducing her output of ore for some years, consequent upon the rapid exhaustion of that famous deposit of blackband ironstone on which her ferruginous prestige has been reared. These districts may, of course, be able to maintain a kind of parasitical influence for years by importing largely from other districts, as they have actually begun to do; but in the last resort they must yield up the sceptre to such districts as can claim to be more richly endowed with the resources which they lack. The most promising districts in the future are more distinguished for the cheapness and abundance than for the quality of their ores. For steel-making purposes and the production of high-class iron, the resources of this country are exceedingly meagre. Even in Cumberland, where the most extensive deposits of hematite iron known in this country have originated the marvellous fecundity and enterprise of Barrow-in-Furness, there has recently been a declension of supply, and the future of the hematite iron trade in regard to local supplies of ore is becoming a source of no little anxiety. The ores of the West Riding, out of which the renowned bands of Bowling and Low Moor have been evolved, are assuming attenuated proportion; and in the dales of Durham, where the spathose and sparry ore has been worked for a number of years by the Weardale Iron Company, the supply has so far given out that the quantity worked is now diminishing year by year. There are several districts, it is true, where large quantities of superior ore are still known to exist; but it would not pay to work the ores of such localities as Wiltshire, Cornwall, and Devonshire on a large scale, considering their long distance from a suitable supply of coal; and the result of the past few years leaves little scope for belief in an augmentation of supply from these sources. It is increasingly probable that the great bulk of our high-class ores will henceforth be imported from Spain and elsewhere; and this, although comparatively a new trade, has already attained very respectable proportions. As a single proof of the potential direction of this fact we may mention, that in the Cleveland district large works are now being built to smelt Spanish hematite for steel-making purposes, in the immediate locality of mines capable of producing the cheapest iron ore in this or any other country; and if "carrying coals to Newcastle" has heretofore been regarded as typical of the greatest ano-

maly, it will hereafter be paralleled by the still greater anomaly of carrying ironstone to Cleveland.

It is desirable that we should pursue this subject one stage further, and take cognizance of some recent features of our pig iron trade. To insist upon the importance of this branch of our national industry would be entirely supererogatory after what we have already stated; but in a single sentence we may affirm, that for some years past Great Britain has produced about one-half of all the pig iron made in the world; that in and through the pig iron trade employment is furnished to fully half a million hands, representing a total population of at least two millions; that pig and finished iron rank in the Board of Trade returns as the most valuable of our manufactured productions, and that they jointly absorb more labor, more *matériel*, more ingenuity, and more capital than any other industries, whether at home or abroad.

We have not yet at command the detailed figures relative to the production of pig iron in 1876, but, from the most authentic sources available, we have sought out the actual returns of that year, and here we present them, along with an abstract of the production of the different districts enumerated for the years 1871 and 1875 :

Name of district.	1871.	1875.	1876.
	Tons.	Tons.	Tons.
Northumberland	34,165	22,870	
Durham	759,244	786,206	2,070,000
Yorkshire, North	1,029,885	1,240,243	
Do. West	114,549	267,153	—
Derbyshire	270,485	272,065	—
Lancashire	520,359	558,780	520,000
Cumberland	336,569	486,112	400,000
Shropshire	129,467	120,996	—
Staffordshire, North	268,300	241,398	220,000
Do. South	725,716	470,540	390,000
Northamptonshire	60,512	80,689	—
Lincolnshire	30,122	111,683	—
Gloucestershire and Somersetshire	99,997	59,819	—
North Wales	41,893	55,099	—
South Wales	1,045,916	541,809	420,000
Scotland	1,169,000	1,050,000	1,103,000
Other districts	—	—	1,027,000
Totals	6,627,179	6,365,462	6,150,000

The figures for 1876 are, of course, subject to the modification of more accurate knowledge, but we believe they will be found pretty near the truth, and assuming their approximate correctness, it follows that our pig iron trade has declined to the extent of about half a million tons, while its value has diminished to the extent of nearly two millions sterling since 1871. There is another curious fact germane to this inquiry. The number of furnaces in blast fell from 673 in 1871 to 629 in 1875, while the nnmber of furnaces in blast during 1876 must have been still less.

How far the results of the last two or three years may be affected by the events of the future it is impossible to foresee or anticipate. The prohibitory price of iron of all kinds in 1872–73 stimulated the development of the resources of other countries, which would otherwise in all probability have been content to depend on England for their supplies of iron, and hence we find that over-production has caused a crisis in other countries as well as at home. This is more particularly true of the United States, where the make of pig iron has steadily declined for the last two years; and the iron trade of this country has certainly the satisfaction of knowing, that if production has progressed backwards at home it has not been otherwise abroad. But then England is different from other nations in respect of having resources superior to most, and a prestige and supremacy far above all, which justify the reasonable expectation that we should be the last to feel the pinch

and the first to retrieve losses. It is impossible to overlook the fact that important changes have recently influenced the iron trade of Great Britain—changes the full import and significance of which are hardly yet understood, and to which we have not quite become reconciled. Foreign competition will assail us more and more. It is now defensive, but will ultimately become doggedly aggressive, and in some quarters it will more than tax our energies to hold our own. There is hardly a single European nation that is not trying to become independent of our wares. Some countries have wrested orders from our grasp, and there is no more singular phenomenon of trade than that presented by the Belgians—when they import our pig iron and work it up into the finished article at a price that enables them to undersell us in our own markets. Cheap and nasty commodities are falling out of repute. The enormous requirements of the railway world will not henceforth, as heretofore, be satisfied by iron rails made from the most coarse and silicious ores that England can show, but must more and more demand a superior metal, made from the best ores, and of such ores England is certainly not possessed to a greater extent than some other European nations, who have the superior gain of cheaper labor and closer proximity to large and lucrative markets. On the other hand, however, England can always depend upon being able to produce a cheap quality of iron fit for many of the ordinary purposes of commerce, and capable, by chemical and other processes now being evolved from the region of experiment, of turning out a much better product than anything yet realized.

EXPLOSIONS OF STEAM BOILERS.

By JOHN W. HILL, M. E., a Member of the American Society of Civil Engineers.

Contributed to VAN NOSTRAND'S MAGAZINE.

THE alarming frequency of explosions, especially in the rural districts, demands that the attention of State Legislatures be directed to a speedy solution of the important problem of safety in the use of steam boilers. The interest of the public in a proper system of inspection of steam boilers is rapidly developing, and the necessity of such a surveillance of the manufacture and operation of this eminently useful and dangerous adjunct of civilization, as will reduce explosions to a minimum, is probably felt by all, however remotely interested in steam machinery.

What is required is the appointment of a Board of Inspectors in every State, to investigate and report upon every explosion, as well as to pursue a rigid system of inspection of the construction and use of steam boilers.

Whilst it is not imagined that such a Board could enter upon their duties sufficiently charged with information to prevent all explosions in the future, their association with the work from year to year, and by frequent exchange of views with other similar Boards, would presently expand and develop their knowledge in a manner not to be attained by other processes.

It appears to the writer that the appointment of an Engineer with a selected corps of assistants, to inspect all boilers now in use and recommend legal measures for the prevention of disastrous explosions in the future, would be quite as desirable a "luxury" as the usual Geological corps, for whilst the labors of the latter may improve our knowledge of the physical structure of our respective increments of the sphere, and open up avenues to unexpected wealth, the labors of the former will save priceless lives and property to the extent of millions.

That there are certain political objections to the inauguration of such a system is admitted, but the combined wisdom of our State law makers should be sufficient to meet the "legal" and "moral" impediments to a rigid law regulating the manufacture and use of steam boilers.

However this may be, no one who is a constant reader of the metropolitan daily papers can doubt the necessity of a care-

ful system of inspection of the materials and workmanship employed in the construction of steam boilers, and in the use of the boiler after it is set to work.

The great majority of accidents are not with boilers in the hands of men who, from the force of circumstances, are supposed to have a certain knowledge of the "regimes" to be established in operating a steam boiler, but with the rural steam users whose knowledge is naturally very "limited," and as naturally very "dangerous." Whether the frequency of explosions in the "country" is the immediate result of the lack of appreciation of the dangers surrounding a seething boiler, or to impositions practiced on the unwary by knavish boiler makers in furnishing poor workmanship and defective materials, is a question to be determined. That poor materials and workmanship are often the "prime" cause of disastrous explosions is well known, and however this may be, a system of rigid inspection, by competent officials in every state, would speedily bring the construction and use of steam boilers to the proper level.

The system of inspection should embrace: The form of boiler as affected by the water of the locality in which it is to be used; The variability of load, and the fuel to be burned in the furnace; The dimensions as affected by maximum capacity required; The thickness of plates, class of riveting and caulking, and quality of iron to be used as affected by maximum pressure under which the boiler is to be worked; The test to be applied to the iron used, and the tests to be applied to the finished boilers; The manner of heating and purifying the feed water and its introduction into the boiler; The style of furnace to be used and general arrangement for facility of inspection; The safety appliances, and standard of tests for "steam gauges," "safety valves," low water alarms and other devices applied to steam boilers.

Every steam boiler now in use, and every steam boiler made in the future should be subject to inspection, and a "seal" put upon it, and a certificate with restrictions under which it may be worked, furnished the owner, tampering with the one or exceeding the other to be visited with a severe punishment.

In France a manufacturer cannot put in use a steam boiler without a permit from the prefect of the department. In making an application for "license" to purchase and put to work a steam boiler, the manufacturer addresses the prefect on a government blank furnishing the following information: Maximum pressure of steam under which the boiler is to work; Horse power and class of connected engine; Form of boiler desired; Location of boiler in relation to buildings and public highway; Fuel to be burned; Nature of business conducted in the establishment, and plan of location (on separate sheet).

The prefect of the department refers the application to the prefect of the *arrondissement*, who in turn refers it to the mayor of the *commune;* this officer then proceeds to an investigation *de commodo et incommodo.* The investigation is continued for ten days; five days after its termination the mayor addresses the *proces-verbal* of the investigation, with his recommendation in the premises, to the prefect of the arrondissement who transmits with his opinion to the prefect of the department. The prefect then lays the *proces* before the nearest government engineer who examines and delivers an opinion upon which the decision of the prefect is based. This decree of 1810 (which as the writer is advised is still in force) in connection with an ordinance passed in 1843 relating to steam boilers, which provides that the boiler shall be tested—first, at the shop where it is built; second, at the establishment where it is to be used—by the nearest government engineer, who after inspection furnishes the owner a certificate of condition and restrictions under which the boiler shall be operated. The tests are *obligatory* (except for mines) and give the manufacturer an *immunity* in the use of a steam boiler nowhere else approximated.

Under our system, or rather lack of system, the manufacturer buys and operates his boiler at his own option; if he desires to drive a forty horse power engine with a twenty horse power boiler, there is no law so far as the writer is aware to prevent his doing so.

The one great impediment to procuring a legal enactment relating to steam boilers is the general indifference of the public to the safety of human life. Take the preceding instance: If in carrying

out the intention to drive a forty horse power engine with the twenty horse power boiler, the boiler "lets go," the public sympathy would be as great for the man who lost the boiler as for the men who lost their lives.

As an illustration of this, the writer would relate a circumstance happening several years ago:

A steam boiler, furnishing power to a very large agricultural machine shop, exploded with terrible violence, demolishing one entire section of the building, and killing and injuring several of the workmen; the writer, coming on the ground a few minutes after the explosion, saw the workmen bearing off the corpse of one of the victims. Shocked at the sight, and desirous of ascertaining the extent of damage to life and limb, he suggested to a bystander "that it appeared to be a very rough accident;" the response came in a suppressed tone, "It was rough on Smith, he would be obliged to buy a new boiler." In this instance six men were killed and perhaps twenty seriously injured. It may not be out of place to remark that this was one of those rare cases where the engineer enjoyed the princely income of "six dollars a week."

In nearly every instance of boiler explosion, it appears that the usual legal investigation of the causes of the accident is a mere "farce," that neither determines the real or proximate causes, or locates the blame where it properly belongs; and whilst the facts usually adduced at the inquest may form a foundation upon which the experienced engineer can build a theory of explosion, it is in the great majority of cases simply absurd to base a legal verdict upon the opinion of men whose knowledge of the steam boiler is of the most limited kind.

Several years ago a small cylinder boiler furnishing steam to a "digester" in a large soap and candle works in Cincinnati, suddenly "let go," killing the attendant and one of the factory hands on the spot; while a section of the shell weighing upwards of a thousand pounds passed directly up two or three hundred feet, thence westward nearly a half mile and fell killing three small children.

At the inquest it was ascertained that no one but the attendant was to blame, and as he was already dead, the coroner "generously forebore to prosecute him;" at the same time the facts in this case as related to the writer by the previous attendant of the boiler, were such as to have condemned the proprietors to several years penal servitude, under the "boiler law" of Prussia.

Coupled with the lack of legal inspection, the general location of boilers in many of our large manufacturing establishments is reprehensible in the highest degree. In the city of Cincinnati there is a certain establishment covering a superfice of 300×200 feet, and lifting skywards seven stories.

Each of the floors except the basement, contains a small army of workmen, and thousands of dollars worth of costly materials and manufactured goods in various stages of completion. In the basement about as central as posts and stone pillars would permit, is located the battery of boilers furnishing the power to drive the machinery. Let us suppose an explosion in this case, what would be the probable results? Is it to be imagined that any large portion of the several hundred workmen shut up in this miniature Vesuvius would escape whole? By no means. Let the slender threads now linking safety to disaster loose their hold, and the pent up volcano would burst forth pouring human lava through the vents. Such an occurrence would fall upon the community like a mantle of darkness, and great would be the desire to locate the blame *somewhere*. The coroner would assume an air of marvelous concern, and swear by the party that put him in office, that the affair should be probed to the quick, and the fault brought home to its father, "though angels weep." To this end a jury would be struck, composed of distinguished citizens, with a plentiful *lack* of information upon the questions to be brought before them, who after the usual delays would "on with the quest."

The picture may be highly colored, but the outlines are lifelike, as any one may verify who will read the testimony and verdicts of the inquests following appalling accidents.

The fall of the Dixon bridge, the breaking of the Mill River dam, the Ashtabula horror, and the late total demolition of the Rockford court house, furnish excellent magazines of informa-

tion upon the customary "legal" proceedings following these wholesale murders.

If we could have the inquest before the accident instead of after, how much better it would be; although this might seem "paradoxical," it is the spirit of the French law regulating the use of "steam boilers," and an explosion in that country *is a rare event.*

ON THE BOTTOM-VELOCITY AND THE VELOCITY-SCALE OF RIVERS

By J. SCHLICHTING.

Translated from "Zeitschrift für Bauwesen," for "Abstracts" of Institution of Civil Engineers.

In estimating the water masses of rivers, only approximate values of the velocity of water immediately above the bed have yet been obtained, because the instruments in use do not permit of a direct measurement, and the theories relating to it are not universally accepted.

To remove this difficulty, to clear away doubts respecting the bottom velocity, and to find a law applicable to all streams, numerous measures were made in the years 1873 to 1875 inclusive, and it has been found that the bottom velocity at any point can be determined where the velocity has been measured by instruments at two known points vertically above it.

With the exception of Hagen all recent writers on this subject assume the velocity scale to be a parabola with axis horizontal and under the surface, while according to observations of A. Gasse on the Elbe it has been found on the surface.

The Author remarks that the results of the American experiments are founded upon an arbitrary grouping of experiments, and are consequently of no value, and that the only essential differences now remaining are whether the maximum velocity lies in the surface or below it, and whether the axis of the parabola is horizontal or vertical.

The Author used Woltmann's current meter for all measurements of velocity. For surface velocities one edge of the vane just touched the surface of the water while the opposite edge was 7.8 inches (20 centimètres) below it, and the resulting velocity is regarded as that at the depth of 4 inches (10 centimètres). The velocities lower down were taken at intervals of 19.7 inches. Five cross sections of the Memel were taken near the bridge of boats at Tilsit, and were plotted on paper, the most suitable points being chosen for the velocity measurements, and marked by buoys. A pole 20 feet long was driven into the bed of the river, so that the current meter might slide up and down, and be suspended at any required depth by a light chain. Velocities were taken at 550 points in 130 different verticles. Each velocity was taken three times, during 100 seconds, and the arithmetic mean was adopted as the true velocity. The maximum velocity was found 80 times in the surface, or at the highest point of measurement, 4 inches under the surface; 5 times, simultaneously, in the surface, and 19.7 inches below the same; 35 times at 19.7 inches below the surface, and 10 times at 39.4 inches below the surface. The minimum velocity was found at 125 times at the lowest point of measurement, and 5 times at points from 11.8 inches to 19.7 inches higher.

The Author concludes that the velocity continually diminishes from the surface to the bottom, and it would follow a mathematical law if the irregularities in the velocity of running water could be eliminated. These irregularities near the shore produce whirlpools, sometimes even reversing the direction of the revolution of the vane.

Two Woltmann's current meters were used and always showed small and variable differences, but similar small variations were noticed when a single instrument was employed. The Author attributes these irregular variations to the irregular motion of the water, and to the inaccuracy which always attends velocity measurements with the Woltmann meter, and to the imperfection of the instruments themselves.

He observes that the formulæ for de-

termining the value of a revolution of the vane are open to weighty objections, and prefers to use the simple formulæ for the velocity of the water,

$$v = \beta n ;$$

where β is the value of one revolution of the vane taken from a number of experiments in still water, and n the number of revolutions in a second of time.

He remarks that if the velocity of the boat used for testing the current meter in still water be the same as the velocity of the stream to be measured, the above formula is the only correct one, and that in all other cases the unavoidable errors of observation render any co-efficient which might be introduced of little value.

If the motion of the water suffered no resistance from the friction of the bed, and from the pressure of the air upon its surface, the velocity scale would be a vertical straight line, because the velocity would be the same, the pressure and the fall being the same.

But on account of these resistances the motion of the lowest layer is retarded by the friction of the bed, and this retardation will affect the motion of the next layer, and so on to the surface. The retardation gradually diminishes from the bottom upwards, and the form of the velocity curve or scale would appear to correspond better with a parabola with its axis vertical and its vertex in the bed, than with a parabola with horizontal axis on or near the surface. The resistance due to the pressure of the air upon the surface of the water may modify the form of the velocity of the scale, supposing the air to be still; but no known instrument is capable of giving such small differences of velocity as would be required to establish this point. Floats will not suffice for this, because, besides their immersement, they have a part of their surface in the air; also they give only the mean velocity in a certain length, and not the velocity at any single point. It is probable that the resistance of the air at the surface of the water acts in the same way as the friction of the bed, but in a much smaller degree.

This would lead to the conclusion that the velocity scale was in its upper part a parabola with its axis horizontal and under the surface, and that this secondary parabola would meet the principle one in an unknown point not far below the surface.

This curve, composed of portions of two parabolas, may be the true velocity scale when water is flowing with steady motion, and the air is still;—the velocity scale being accepted as a parabola with its axis vertical, and its vertex in the bed.

After reviewing various formulæ, the Author sums up as follows:—

1. The mean velocity scale of rivers for each cross section, as long as it does not present any striking irregularites of depth, is a parabola with the axis vertical and the vertex at the bottom. The scale is obtained from average values of direct measurements of velocity, and consists of a rectangle contained by the mean bottom velocity as base, and the mean depth as height, and of a semi-parabola of the same altitude. Two average values suffice to determine the velocity scale, and also the bottom and mean velocities, and the parameter of the parabola.

2. The maximum velocity in a stream, not influenced by wind, storm, ebb, or tide, lies in the surface or very near it, as well in the mean velocity scale as in the separate verticals, excepting those near the shores.

The minimum velocity is directly above the bed, and the mean at $\frac{1}{4}$ of the depth above the bed.

3. The velocity at any depth is equal to the bottom velocity increased by the ordinate of the parabola corresponding to the depth.

4. The ordinate of the parabola at $\frac{1}{3}$ of the depth above the bed is to that in the surface as 2 to 3, if the resistance of the air be disregarded.

5. The bottom velocity at any point depends on the fall, the depth, and the nature of the river bed. The value of the retardation is at least as great as the value of the ordinate of the parabola which lies in the surface.

6. In consequence of the variable bottom velocity, the relation between the bottom and mean surface velocity cannot be constant either in different verticals, or in the same vertical with different heights of water. Also the relation between the parameters of the parabolas belonging to the different velocity scales cannot be constant.

7. Such constant relations cannot be de-

termined even by making use of co-efficients, because the latter vary more or less according to the resistance of the bottom.

8. The frequent change in the form of the velocity scale, based on the variable resistance of the river bed, explains the unequal movements of water in rivers. As long as the river bed is irregular and changeable no universal law can be determined for the motion of the water.

The methods hitherto used for the calculation of the water masses of rivers, from the measured current velocities in various verticals and at different depths, lead to inaccurate results.

Water masses may be more correctly calculated by dividing the cross section of the stream by vertical lines at numerous points, and determining only the mean velocity at each point at $\frac{1}{5}$ of the depth above the river bed, from the arithmetic mean of at least three observations, and then summing up the products of the mean velocities thus found, and the separate areas of the cross section.

The original paper is illustrated by two plates: one shows the five cross sections of the Memel taken near Tilsit, and the other contains diagrams in illustration of the velocity scales obtained from the measured velocities at the above cross sections.

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—The last number of the Transactions contains:

Discussions on the failure of the Ashtabula Bridge, W. M. Roberts; Relative quantities of Material in bridges of different kinds and of various heights, C. E. Emery; The Flow of Water in open Channels, T. G. Ellis; A Novel Railroad Survey, T. S. Hardee.

THE MECHANICAL ENGINEERS AT BRISTOL.—The summer meeting of the Institution of Mechanical Engineers was held at Bristol, and was mainly remarkable for the grave, not to say alarmist, tone of the address of the president, Mr. Thomas Hawkesley. After giving a brief account of the financial position of the institution, and of the arrangements made for its permanent settlement in Victoria Street, the President went to the war question, and spoke of the necessity for this nation to keep open the paths of the ocean in order that we may obtain our food supplies. He condemned, as others have condemned, the building of floating castles, the loss of one of which would be a costly disaster; and he recommended, as others have recommended, the construction of a fleet of light, swift, and well-engined ships, which by the exercise of a daring and hornet like activity, would succeed in driving every enemy's ship from the face of the sea. Then turning to the question of trade, he astonished his audience by stating that "we are at this time diminishing our wealth, and the means of supporting our rapidly-increasing population, by more than one hundred million pounds per annum," simply because foreign nations will not take a sufficiency of our goods to enable us to liquidate our indebtedness for food supplies. According to Mr. Hawkesley, we do not manufacture cheap enough. Labor is too dear, and workmen are at war with their employers. Wages have been unduly forced up, but not the means of living; and trades which were once monopolized have been driven away. Mr. Hawkesley contends that if the evil day is to be postponed, the cost of all commodities, labor included, must be closely assimilated in this kingdom to the cost of like commodities on the continent of Europe and in America. Mr. Bramwell, in proposing the vote of thanks, said that while he hoped some of his hearers differed from the president, he trusted they would all join in thanking Mr. Hawkesley for setting them thinking upon matters of so much importance.

The reading of the papers was then commenced by Mr. J. C. Wilson, who submitted a paper mainly descriptive of a safety-valve designed by Prof. Klotz, of Prague, which, however, did not meet with much favor from the members present, and which Mr. Lewis Olrick declared was almost identical with a valve introduced by Mr. Bodmer upwards of fifteen years ago. The discussion that followed was well sustained, and was highly interesting. Mr. Webb, of Crewe, then read a paper on a circular slide-valve, designed by him some years ago, but which has only been in use long enough to fairly test its merits. We will merely say here that the valve is made circular, and is free to revolve in its buckle, so that if there is more friction in one part than in another, the valve revolves, and the sliding faces are, in consequence, equally worn, grooving being effectually prevented. The valve is applicable to either steam or hydraulic engines, and samples which have been severely tested were exhibited, so truly plane that if the surfaces were wetted one would support another. Mr. J. G. Geach gave a description of the appliances used in constructing the heading under the Severn for the Severn Tunnel Railway, and in the afternoon the members visited several works in Bristol and the neighborhood, and attended the conversazione given in the evening by the President and Mrs. Hawkesley at the Merchant Venturers' Hall.

On the following day Mr. T. H. Riches read a paper on the Tynewydd colliery inundation, and described the operations undertaken for the release of the imprisoned miners. This paper was probably the most interesting of those read at the meeting, and it was satisfactory to learn from Mr. Riches that, after the experience gained by the Welsh miners, he thought there would in future be no difficulty

in getting men out in case any accident of a similar character occurred. With reference to the air lock, for enabling the rescuers to work without danger from the rush of compressed air, Mr. Cowper suggested there ought to be a regulation that one should be kept at all collieries. Mr. Froude, F.R.S., described a new dynamometer, devised for measuring the power delivered to screw-propellers of large ships, the well-known friction brake having been found to involve greater difficulties than were anticipated, consequently necessitating the invention of some other instrument to enable Mr. Froude to carry on the investigations directed by the Admiralty. Mr. J. C. Fell read a paper on "Variable Automatic Expansion for Steam Engines," which was mainly descriptive of the working economy of Rider's gear, the promptness and efficiency of which are, according to Mr. Fell, most marked. He said that an engine at a fulling mill in the neighborhood had been fitted with the valve gear, and a crucial test of its powers was made by throwing off both mills simultaneously, when no change in the speed of the engine was perceptible to those watching it. Several papers were postponed for want of time, notably a description of improved radial axle-boxes and guides, by Mr. H. Widmark, of Bristol, and one on special mechanical appliances for use in certain classes of mine accidents, by Mr. C. Hawke-ley and Mr. E. B. Martin.

Excursions were made to the Severn Tunnel works, when the powers of the drilling machine described by Mr. Geach in his paper were ocularly demonstrated to the assembled visitors, several of whom expressed the opinion that it was the best drill yet invented, and that if the present work were carried to a successful issue, it would hasten the construction of the Channel Tunnel. Several other excursions were made, and the summer meeting at Bristol will be long remembered by those members of the Institution who attended.

IRON AND STEEL NOTES.

How Siemens Steel is Made.—At the January meeting of the graduates' section of the Institution of Engineers and Shipbuilders in Scotland, Mr. Jn. Mayer, F.C.S., explained at some length the nature of the materials employed for this purpose, the chemical transformations which they undergo in the Siemens regenerative furnace, in order to obtain the different qualities of steel which are required for rails, sheets, plates, hoops, wire, &c., and he stated that in this country alone works capable of producing by this process 250,000 tons per annum are erected. Dr. Siemens lent some diagrams to illustrate the paper. The discussion was adjourned, so that members who had practical experience in using Siemens steel for engineering purposes might be present to speak.

Danks Iron.—Messrs. Hopkins, Gilkes, and Co., Limited, of the Tees Side Iron Works, Middlesbrough, have succeeded in making, with the Danks rotary puddling apparatus, homogeneous iron exclusively from Cleveland forge pig iron, and this iron will be branded "H. & Co. Danks." The makers state that it is equal in quality to South Yorkshire or best Staffordshire iron. At the Middlesbrough quarterly iron market in January they showed specimens of rails, bars, &c., which had sustained very severe tests. The rails were exceedingly good in fracture, crystalline in structure, and perfectly homogeneous, and the iron was decidedly much better than has previously been obtained from Cleveland forge iron alone. The rails and other iron are made without piling, the whole process of manufacture being followed straight through from the puddling machine to the finished product with nothing more than a wash-heat before rolling off. It will be remembered that Messrs. Hopkins, Gilkes, and Co, were the first in Europe to adopt the Danks furnace, and they exhibited one in operation immediately after the meeting of the Institute in London in 1872, since which time they have been experimenting with the machine, having had to encounter numerous mechanical difficulties. The results are now before the trade. A portion of the works was some time ago remodelled, and about a dozen furnaces erected.

Manufacture of Slag Wool.—Heretofore in the manufacture of this material the hot slag as it leaves the furnace has been subjected to the action of a jet of steam or air for the purpose of dividing it into extremely fine filaments, but the direct action of the steam has not been altogether successful in the production of a material free from impurities known generally in such manufacture as shot. Mr. Chas. Wood, of Middlesborough-on-Tees, has, therefore, devised means whereby he believes a large proportion can be made entirely free from shot, thus leaving the fibers or filaments almost pure. He conducts the slag from the furnace by the usual slag runner and at the discharge end, and on one side underneath the usual slag runner he places an air or steam jet, preferably the latter; on the other side of the runner, and opposite the steam or air jet, he provides a large tube of wrought or cast-iron leading to a chamber or receiver, to be hereafter described. The mouth of the tube—that portion of it near the runner—is open on the lowest side, so that the shot coming from the slag as the wool is divided from the same, or in other words, as it is manufactured, is free to fall to the ground or into any suitable receptacle, while the slag proper goes into an ordinary slag box, or is otherwise disposed of as desired. Into the tube and beyond that part thereof which is not open he leads a second jet for the passage of air or steam, and the object of this second pipe is that the air or steam which is forced through the same toward the chamber draws the wool or silicate cotton which has been produced by the first steam or air jet into the tube, and sends it on into the chamber; this chamber is constructed or formed of a series of frames made of wire netting for the purpose of catching the wool blown into the same, and allowing at the same time the steam or air to escape.

With regard to the arrangement of the wire

netting, he finds it most convenient to have them in a V or corrugated form, and to connect to the apexes of (say) the V's what he terms draught plates, which tend to check the current of air or steam, and to allow the fine qualities of the wool to settle behind them in the angles formed by the V-shaped netting. Near the entrance of the chamber, and opposite the tube, he places a board or plate for the purpose of arresting any shot that may possibly be carried into the chamber through the tube, and thus stop the shot from contaminating the wool. A galvanized iron or other roof may be provided for the cage or chamber, but this may also be of wire netting or any other material. The invention then essentially consists in the employment in the manufacture of two air or steam jets, one to make the wool, and the other to draw it into and send it through a tube into a chamber or cage made of wire netting or perforated plates, or equivalent material, in such manner that the wool is caught by the sides, while the air or steam is able freely to escape without forming currents, and also a great side area of netting or perforations upon a comparatively small space of ground. The simple but effective means provided for arresting the shot, and dividing it from the wool, is also a very important feature in the invention. —*London Mining Journal.*

RAILWAY NOTES.

RARCHAERT'S TOTAL ADHERENCE LOCOMOTIVE.—The scientific and technical portion of a late number of the *Annales des Mines*, is wholly taken up by a memoir of M. Massieu, and a short note of the inventor, upon Rarchaert's "locomotive of total adherence and converging axles." The locomotive was first tried on a circuit of 58 kilometres, during which it behaved well in all points of view, reaching a mean velocity of 40 kilometres, and sometimes exceeding 50 kilometres per hour, on slopes of .015 and in curves of 250 metres radius. After this trial, it was employed, for about two months, upon trains. M. Rarchaert finally obtained authority from the company to employ his machine on regular trains, under the charge of the company's engineers and stokers, the expenses being borne by him. This condition was so burdensome, that he terminated the trial at the end of a month. During that time the locomotive had traveled 4349 kilometres, and had satisfied all requirements without accidents. It has since been employed on the road from Orleans to Chalons. M. Massieu and his principal subordinates carefully studied the details of construction and operation, and presented two successive reports, which he was invited, by the commission of the *Annales des Mines*, to embody in a single memoir, under the following heads:

1. Brief examination of the processes hitherto employed to facilitate the pass ge of locomotives on curves, with the more or less complete utilization of the adherence which their total weight can give.
2. Description of the apparatus.
3. Study of the apparatus in a kinematic point of view; examination of the conditions in which it can overcome the curves and irregularities of the track.
4. Study of the apparatus dynamically; examination of the causes which can impair its stability.
5. Comparison with other locomotives employed on secondary lines; results of the trials.
6. Examination of the objections against the use of a single and straight connecting rod.
7. Summary and conclusion.

The memoir covers 200 pages; M. Rarchaert's note, 12 pages. Both terminate with the following conclusion: "For a long time, the constructors of locomotives with a single motor, have tried to separately realize, sometimes total adherence, sometimes flexibility; sacrificing, according to circumstances, one of these conditions to the other. Among those who have tried to realize them both at once, I am led to believe that M. Rarchaert is the one who has best, and even for the first time, practically succeeded."

CONTINUOUS RAILWAY BRAKES.—A valuable report was recently forwarded by the Belgian Government to the British Board of Trade, on the subject of continuous railway brakes. The Administration of State railways in that country, fully aware of the importance of obtaining the best available means for the control of trains, have since a long time given a fair trial to various systems of brakes which promised to give good results. Experiments were conducted with the Achard, Masin, Heberlein, and others, which however were thrown aside, and careful attention was given to the system which in 1873 appeared the most perfect that had yet been introduced. This was the Westinghouse atmospheric brake, which had previously been tried in this country, and which has long been working with excellent results upon the Metropolitan District Railway. The experiments with this system showed, as might have been expected, a superiority as compared with the hand brakes of about 3½ to 1. The experimental trains passed into regular service, and showed consistently their efficiency and reliability. The defects of the system were however fully appreciated by the Railway Administration, the chief one being the delay that arose in bringing the brake into action. On the other hand, they equally appreciate the advantages it possessed, and which they summarized under the following heads: 1. Comparative promptness of action. 2. The facility of gradual application, and the fact that the driver of train could control the brake. 3. The distribution of elastic pressure on all the wheels throughout the train. 4. Its applicability to every wheel in the train. 5. Its non-liability to derangement. 6. Its action being independent of the motion of the train.

Between 1873 and 1875 financial reasons prevented any further adoption of the system on the part of the Belgium Government, and in the course of that year the new form of brake perfected by Mr. Westinghouse, and known as the Automatic, was submitted to the Administration, and at the same time another system which had also been acting, brought

into notice—the Smith vacuum—was submitted to the consideration of the Belgian railway authorities. It was therefore decided to disregard the various other systems which showed more or less promise, and to confine attention in the new series of experiments solely to the automatic and vacuum brakes. Accordingly two similar trains were fitted with these apparatus, and a number of carefully conducted experiments were made with them, and which are recorded in the report. The comparative results obtained corresponded very closely with those deduced from similar trials in this country, but it will be noticed that with neither brake did the stops made approach those attained on the North British Railway. It is to be regretted that the times occupied in making the stops are not recorded in the report. It will be noticed also that all the experiments were made at low speeds, so that the respective capacities of the two systems were not by any means developed as they have been in this country. Still, as will be seen in the report, the results were so decidedly in favor of the automatic brake as regards its promptness and efficiency, that the Committee reported unanimously in favor of its adoption, and acting on this view, the Administration has accepted the Automatic as the standard brake throughout all the State lines, and its application will be commenced immediately. The Committee are also unanimous in their conclusion that the system they have proved to their own satisfaction, to be so superior to the vacuum brake, that they recommend it, in spite of the greater cost, will prove far less expensive in maintenance and working than the latter, and they take pains to point out that, judging from the short experience obtained, and from the nature of the brake, that the apparent advantage in first cost is "more deceptive than real, the cost of maintenance being higher for the Smith brake than for the Westinghouse, either automatic or atmospheric;" while the quantity of steam required to work the first-named is greater than for either of the other two, and therefore the cost of each stop made is greater.

The Belgian Government in taking this prompt action in the brake question, are to be congratulated upon the selection they have made. Whatever may be the drawbacks to having the railway system of a country vested in one central Administration, such an arrangement has, at least, this advantage, that it is free from all the conflicting interests and opinion, which in England render concerted and uniform action almost, if not quite, impossible. This is illustrated by the Government publication just issued containing the correspondence between the Board of Trade and the Railway Companies Association, in which the representatives of the great lines of this country all advance conflicting statements, relative to the efficiency of various brake apparatus.

We believe that a series of trials with continuous brakes is shortly to be carried out in Germany under Government auspices, and we trust that they will be conducted in such a way as to develop to the utmost the full capacity of all the different systems competing, that is to say, that the trials should be exhaustive, and made with similar trains, consisting of at least ten or twelve carriages. So far as this latter feature is concerned, we believe it has been the intention of the railway authorities only to fit up four or five carriages on each system, but it need scarcely be pointed out that so limited an application would fail in giving anything like conclusive or satisfactory results.

ENGINEERING STRUCTURES.

THE bridge of the new Cincinnati Southern R. R., over the Kentucky river, is said to be the highest railroad bridge in the country. It is 275 feet high, having three spans, the middle one 375 feet long, and the others 300 feet each, the total length being 1,125 feet. There is a bridge in Switzerland which is 254 feet high, but with a span only 144 feet long, and one at Verrugas, in the Andes, 252 feet high, with spans 125 feet long. The piers of this Kentucky bridge are the largest in the country, except those at Brooklyn, the stone work being 130x47 feet and the base of the iron work 117x28. The frame is all wrought iron, and was built out from the abutments toward the center of each span.

THE HUDSON RIVER TUNNEL.—It is the intention of the Tunnel Company to begin work early in the coming fall. A shaft twenty-eight feet deep has been dug at the foot of Fifteenth Street, Jersey City, and this depth will be increased twenty feet. From this as a starting point, the tunnel will proceed in a northeasterly direction under the Hudson River and the Christopher Street ferry slip. The entrance on the New York side will be in the neighborhood of Washington square. From Jersey City the grade will descend two feet in every hundred feet, until a point 2,700 feet from the New York side is reached, when it will begin to ascend at the rate of one foot in every hundred feet. The tunnel will be two miles in length, with a road-bed twenty-three feet wide, and two separate tracks. Through its entire length it will be lighted by gas. The wall will be constructed of brick, with a thickness of four feet. At no point will the top of the tunnel be less than thirty-five feet below the surface of the water, and in many places it will be seventy feet below. One hundred and twenty laborers will be engaged in the construction of the tunnel. The work will go on during the whole of the twenty-four hours, the force working in three relays, for eight hours each. Although the tunnel will be used for the conveyance of passengers, its main object will be the transportation of freight to and from the great railroad lines which terminate in Jersey City. The capital of the company is $10,000,000.—*Iron Age.*

THE CHANNEL TUNNEL.—The engineers of the Channel Tunnel—MM. Polier and Lapparent—in a report upon the result of the numerous soundings which they have taken during the last eighteen months for the purpose of ascertaining the thickness and impermeability of the different strata in the Channel, state that "observation shows that the soil

under the sea preserves the same characteristic as upon the French mainland—that is to say, a line traced upon the surface of one of the beds of chalk consists of long straight lines connected with one another with very pronounced sinuosities. Observation has also shown—and this is a capital point with regard to the making of the tunnel—that in these sinuosities, of which there are two in the Channel, the strata are evidently continuous, and that the distance between the two successive lines is filled up, not by a fissure, but simply by a curve. There is, therefore, no reason for anticipating that the work will encounter any geological difficulties, properly so called, if we bear in mind the information furnished by a study of the sea's bed and by the work done in the mines which run down into the chalk. The boring at Sangatte has not been carried far enough to admit of any definite opinion being given as to the jurassical development of the soil in that region, but it has confirmed the deductions drawn as to the progressive diminution in thickness of the strata below the chalk which runs from Boulogne towards Calais. These strata are formed of sand and clay, with an admixture of palæozoic pebbles, red calcareous and carboniferous marl, but without any rock of the coal order."

THE Wrought Iron Bridge Co., of Canton, Ohio, has the contract for building the bridge over the Connecticut river, at Northampton, Mass. The bridge has a total length of 1219 feet in eight spans with 18 feet roadway, and will cost about $27,000, exclusive of flooring. This company is also building a 930 feet bridge in six spans, with 16 feet roadway, at Columbus Junction, Iowa, being the longest highway bridge in the State, and has the contract for six 120 feet spans, with 18 feet roadway, and 5 feet sidewalk at Paris, Ont. They have just completed a 160 feet bridge, with 30 feet roadway and two 8 feet walks, on iron piers 25 feet high, at San Jose, Cal., and are building a 256 feet span, with 18 feet roadway, at Preston, W. Virginia; all of the above bridges being on the Company's patented truss plans with all wrought iron details. The Company have now over 12,000 feet of bridging in process of construction, and are running their works day and night, giving employment to over 300 men, and are making extensive additions to their shops and machinery to meet their increase of business.

ORDNANCE AND NAVAL.

MR. BRASSEY ON GUNBOATS.—The following letter has been addressed by Mr. Brassey to the editor of the *Times*:—

Sir,—It may not be uninteresting to many of your readers to know that the two gun vessels recently constructed by Sir William Armstrong's firm for the Chinese Government were at Aden on the 16th of April, having performed their long voyage from England satisfactorily.

It has been urged most strongly by Mr. Barnaby that every monster ironclad should be supported by a flotilla of gunboats. Has due weight been given to this suggestion of the Chief Constructor, which received the strongest support from Sir Spencer Robinson and other authorities?

As skirmishers, in combination with the monster ironclads of the Inflexible type, gunboats of the type designed by Mr. Rendel would be invaluable. In addition to the attendant flotilla of gunboats, each armored ship should carry torpedo boats of great speed. When it is remembered that the blow from a single torpedo would prove fatal, and how improbable it is that such tiny craft, steaming at the rate of 17 knots, and enveloped in smoke, will be struck by heavy projectiles, it can scarcely be doubted that victory would incline in favour of that fleet which would possess the greatest number and the most effective of these light armed naval skirmishers.

Again, considering how impossible it is to construct an invulnerable ship, and that the costliest ships are almost as liable to destruction as those of a smaller and less costly type, ought it not to be a cardinal maxim with naval constructors and administrators to distribute the strength of the navy into as large a number of ships as may be, taking care, of course, that no ship shall be built which is too small to be thoroughly effective in its own particular class? Let us seek for the best practical application of this principle.

The expediency of adding largely to the dimensions of an armored steam ram, for the purposes of mounting an armament of two or four 80-ton guns is open to question on another ground. A captain may hesitate to open fire, and to obstruct his field of view by the smoke of a heavy cannonade, when he sees a cloud of torpedo boats hovering round him, only deferring their fatal assault until their movements are rendered invisible to the enemy by the smoke from his own guns.

How, then, are we to meet these various and complicated conditions with which the naval constructor has to deal? It can only be done by adopting distinct types of ships for the use of the gun, the ram, and the torpedo respectively.

The artillery of the fleet should be mounted on floating gun carriages of the Gamma type, designed by Mr. Rendel. For the torpedo each large ship should carry two or more of Mr. Thorneycroft's swift launches. For the purposes of ramming, a swift armored ship of handy proportions is required; and, with the view to a limitation of size, and in order to secure that quality of handiness so vitally necessary to an effective ram, these vessels should not be encumbered with armor-protected guns.

Let progressive, unprejudiced naval officers compare the kind of force that could be created for a given sum of money, if constituted, according to the suggestions here offered, with a fleet composed of vessels of the Inflexible type. I assume that armored vessels can be built for £50 a ton. The Inflexible type, in round figures, has a tonnage of 10,000 tons, and costs £500,000. Five millions sterling, therefore, would produce only ten Inflexibles, which, powerful as they are, possess no special defence against the torpedo, are armored with penetrable armor, and together carry only forty guns. A fleet of

Inflexibles, it will be remarked, costs £125,000 a gun.

I venture to believe that a like sum of £5 000,000 might be much more effectively applied in the construction of the following vessels :—

1—Thirty armored steam rams of 2000 tons without guns, costing, at £50 a ton, each £100,000.....	£3,000,000
2—Sixty gun vessels of the Gamma type, armed with one 38-ton gun, two 12-pounder breech-loading guns, and one Gatling gun. Each £25,000.....	1,500,000
3—Launches on Mr. Thorneycroft's plan, and other descriptions of offensive torpedoes and torpedo boats.....	500,000
	£5,000,000

For operations in European waters, in the Mediterranean, the Red Sea, the Straits of Singapore, in short, along the whole line of our communications with the East, such a fleet as I have indicated would, in the hands of dashing commanders, be more effective than ten Inflexibles. Is the ram, as some think, the most formidable weapon of the navy, and is the quality of handiness the first condition of efficiency? Then thirty rams are matched against ten, and the smaller vessels, being more handy, are the more effective. True it is that the Inflexibles could steam faster than the smaller rams, and could therefore place themselves beyond their reach; but such powerful vessels were surely not built to run away from an enemy.

Is the gun the weapon on which we can rely? In the plan suggested eighty guns are carried as against forty; and they are, for many purposes, mounted, so as to be more available than if carried in ships of the Inflexible type. For bombardments and for coast defence, a shallow draught is often essential in the vessels employed. It may be necessary that they should pass over shoals or navigate shallow channels. St. Petersburg and Cronstadt were inaccessible to our line-of-battle ships. They might have been destroyed by gunboats. Take, again, the question of coast defence. What could be done to protect the Thames, the Mersey, or the Humber, by ships drawing 30ft., in comparison with the services which could be rendered by gunboats capable of navigating the in-shore channels and threading their way through the intricate maze of mud flats and sand banks with which the estuaries of our largest rivers are beset?

In a naval engagement the utility of gunboats would be scarcely less apparent. Having double the number of guns, the chance of delivering a fatal shot is largely in favor of the fleet supported by a flotilla of gunboats. It may be thought that the gunboats could not sail in company with the larger ships. The plan of towing would meet this difficulty. For an example of what may be done with torpedo boats the recent experiments at Cherbourg are conclusive.

Were half the money now so lavishly, and I fear vainly, spent on unwieldy and no longer indestructible or impenetrable ironclads, employed in procuring for the navy the means of destroying them, the naval administrations of other Powers would abandon the construction of armored vessels. They would acknowledge the hopelessness of the attempt to give protection to their fleets by means of armor against the cloud of gunboats, torpedo boats, and rams which we could bring together against them. THOMAS BRASSEY.

BOOK NOTICES.

SCIENCE SERIES, No. 31.

THE SANITARY CONDITION OF CITY AND COUNTRY DWELLING HOUSES. By GEORGE WARING JR. New York: D. Van Nostrand. Price 50 cts.

This subject is only beginning to engage the public attention to the extent to which its importance entitles it. It is fast gaining its true position however. The best authorities tell us that the time is not far distant when every case of typhoid fever or diphtheria, will be a matter for official investigation in all of our large towns. The author of this essay has put in condensed form the leading principles of Sanitary Science, and their practical application to city and country dwellings, and has thereby made the best contribution we have yet seen to the cause of public education, inasmuch as in so small a compass he has presented quite clearly and completely the most advanced views.

FOWNE'S MANUAL OF CHEMISTRY, THEORETICAL AND PRACTICAL. Twelfth Edition revised and corrected. By HENRY WATTS, B.A., F.R.S. London: J. & A. Churchill. For sale by D. Van Nostrand. Vol. I. Price $4.25.

This work has long been considered one of the best books for reference in general chemistry in the English language. Without the ponderous dimensions of the three or four larger works it contained all that was needful for the student, and in general satisfied the wants of the instructor. It had reached its third edition in 1850, and the tenth in 1868.

The additions necessary to fully present the department of Organic Chemistry were so many, that in the present edition the work appears in two volumes. The first including Chemical Physics and Inorganic Chemistry, and the second Organic Chemistry only.

The usual tables and spectrum chart are found in the first volume.

TRAITE D'ASTRONOMIE ET DE METEOROLOGIE APPLIQUEES A LA NAVIGATION. Par G. CHABIRAND ET L. BRAULT. Tome I, Astronomie. Paris: Arthur Bertrand. For sale by D. Van Nostrand. Price $4.00.

This first volume, which is the only one yet received, is divided into four books. The first treats of the form of the earth and the different methods of mapping its surface, concluding with a statement of the general problem of determining a ship's place at sea.

Book Second is devoted to Spherical Trigonometry, Spherical Astronomy, Methods of calculation, and the reduction of observations.

Book Third contains Movements of Celestial bodies; Measure of Time, Astronomical Ephemeridæ.

Book Fourth deals with practical observations and the necessary reductions.

The whole forming a very complete practical Astronomy for students.

It is a beautifully printed royal octavo volume of 460 pages.

SCEPTICISM IN GEOLOGY AND THE REASONS FOR IT. London: John Murray. For sale by D. Van Nostrand. Price $3.00.

The author who assumes the name of Verifier, finds difficulty in following the line of argument of modern geologists, so he refuses to accept the modern theories.

From such reading as we could give the book, we concluded that the author, whoever he is, has given less information in regard to the basis of modern geological science, than about his own peculiar mental constitution.

In the preface we find this clause: "If the book should attract any attention, it is sure to be met in certain quarters with rough usage." We feel confident that no "rough usage" will be inflicted.

A TREATISE ON CHEMISTRY. By H. E. ROSCOE, F.R.S., AND C. SCHORLEMMER, F.R.S. Vol. I. London: Macmillan & Co. For sale by D. Van Nostrand. Price $10.50.

When we state that this volume of 770 pages royal octavo size is devoted to non-metallic elements only, the reader will doubtless conclude with us that this is the first supply of an extensive treatise.

The more important chemical processes relating to non-metals are fully described, and the apparatus illustrated with the best of wood cuts.

We are assured in the preface that care has been taken to present the most recent, exact and experimental data. The names of the authors are a sufficient guaranty of scientific exactness. No one can take exception to the style in which the publishers have put the treatise before the public.

PERMANENT WAY ROLLING STOCK AND TECHNICAL WORKING OF RAILWAYS. By Ch. COUCHE, Inspector-General of Mines, Professor of Railway and General Construction at the School of Mines, Paris. Translated from the French, by JAMES N. SCHOOLBRED, B.A. London: Dulau & Co. Paris: Dunod. For sale by D. Van Nostrand. Vol. I. Price $20.00.

The following is the translator's preface:

"It is but an act of justice to the distinguished author of this work to say, that some years have elapsed since the translation of the following pages were first undertaken. Various circumstances, over which the author had no control, have led to the delay in the appearance of the English edition; and, in consequence, many of the facts scattered through its pages may now appear somewhat out of date, though when originally written they were fresh and full of interest.

"The author has kindly endeavored to remedy somewhat this fault of the translator's by a few notes in the Supplementary Chapter; and also by some remarks there on 'Steel rails,' which have now almost superseded iron ones, though still in considerable use when the pages were first written.

"In order to render this edition of further interest to English readers, the translator has added, in the body of the work, some details, as to the arrangement of the many lines of rail which communicate with each other at Clapham Junction, and also as to a few of the earlier forms of the interlocking of points and signals; and, in the supplementary remarks on Steel rails, he has ventured to insert a few of the interesting facts and opinions which have been of late elicited on the subject of this metal, now become a paramount importance in connection with railways."

The following is the table of contents:

BOOK I, PERMANENT WAY.—Chapter I.—Breadth of Guage; II.—Form of Rails on Non-Continuous Supports; III.—Establishment of Continuity at the Joints by means of Fish-Plates Strains on the Metal in the Rails, Fish-Plates, and Bolts; IV.—Rails on Longitudinal Bearers; V.—Cross Sleepers; VI.—Ballast, Conditions which it should fulfill, Provisional Ballasting of a Line; VII.—Stone Blocks as Sleepers; VIII.—Systems of Metallic Permanent Way; IX.—Special Points in the Permanent Way; X.—Turntables, and Traversers for Carriage Sheds, and for the Open Line; XI.—Manufacture and Delivery of Rails; XII.—Drainage of the Line, Amelioration of Cuttings and Embankments.

The work is to be completed in four volumes, royal quarto.

The first volume which is the only one yet ready is accompanied by a folio atlas of 38 plates.

THE MAGNETISM OF IRON VESSELS, WITH A SHORT TREATISE ON TERRESTRIAL MAGNETISM. By FAIRMAN ROGERS, Member of the Compass Commission of the National Academy of Sciences. New York: D. Van Nostrand. Price 50 cts.

Several years since much interesting discussion took place in the *Mining Journal* concerning the magnetic action of iron in the vicinity of compasses employed in instruments of precision, the great interest in the subject felt by miners arising from the importance of knowledge of this kind to secure accuracy in the underground surveys. Facts in connection with magnetism, which are of interest to those engaged in navigation, are, therefore, of equal interest to miners and surveyors; and a great mass of these facts have been brought together in Mr. Fairman Rogers' treatise on Terrestrial Magnetism and the Magnetism of Iron Vessels, recently reprinted from VAN NOSTRAND'S ENGINEERING MAGAZINE, as one of his Science Series. Mr. Rogers points out that the class for which he writes are interested in terrestrial magnetism, on account of the magnetic needle and its variations with the magnetic action of the iron in the surrounding machinery. The simple experiment of floating a magnetic needle upon water shows by its assuming the usual direction, without moving towards the northern boundary of the vessel containing it, that the

force acting upon the needle is a directive force, not one of attraction for the needle as a whole. Careful experiments have also led to the inference that the compass needle points to the north in obedience to the law which causes it to take a position at right angles to currents which are passing around the earth in a direction nearly parallel to the equator, and from west to east.

To whatever causes we may attribute the action of the magnetic needle, a large number of observations are required to furnish the data upon which the true theory is finally to be founded, and the numerical value which will serve as a basis for calculation. These observations are at present directed to the direction of the needle at the different parts of the earth's surface with reference to the true meridians or geodetic north pole and to the dip of the needle, or the direction with reference to a horizontal plane which a needle accurately balanced before being magnetised assumes after being magnetised. In both these cases the amount of the directive force as well as the position assumed is to be measured. The force acting upon the needle in the direction of the dip at any given place is called the total force of the earth at that place. In other words, the force acting upon the compass needle is not simply one in a horizontal plane directing the needle towards a magnetic pole, but it is a force acting in an inclined direction, which we divide for practical purposes and for convenience of observation and consideration, by a well-known device in mechanics, into a horizontal component and a vertical component. The direction of the horizontal component can be readily observed by comparing the direction of the needle with that of the true meridian obtained by observations upon the north star or the sun, or by any of the means well known to seamen. The deviation from the true north line, or the variation of the compass, differs in different localities and at different periods. The diurnal variation is too small to be of practical importance to the navigator. The variation is of the utmost importance, as in some localities ordinarily visited by the navigator it is as great as 50° to 60°, or four or five points to the west, so that when heading due north magnetic the ship will be sailing N.E., or N.E. by E. Assuming similar variations to occur on land as at sea, miners will appreciate more than ever the importance of all plans of mining and engineering works being laid down to the true north, and as Mr. Rogers' object has been to enable all concerned to allow for the deviation or to correct the compasses, so as to eliminate it, the value of his treatise will readily be appreciated.

Originality of assertions in a treatise of this character would render it worthless, yet the modes of stating facts are so numerous that there is frequently as much newness in technical as in other works, and the value of the former depends much on the clearness with which the facts are stated. Mr. Rogers does not pretend to give any material not previously published, but he supplies such information as will enable an officer, previously unacquainted with the subject, to undertake a series of observations which would be of value in adding to the general knowledge of the matter, or in studying his own ship, so as to avoid mishaps from a too firm reliance upon uncorrected compasses, or from unexpected changes in new magnetic latitudes. With regard to arrangement, there is an entirely new table which conveys as much information in a few minutes as would otherwise require many hours' reading to get together, and then would not be nearly so well impressed upon the memory. To navigators the treatise will prove invaluable (indeed it was originally written for a Manual of Scientific Enquiry, which the Navy Department proposed to publish, though the intention was never carried out), whilst mining officers will find a vast amount of information which it will be to their material advantage to possess; it may be hoped, therefore, that the volume will be widely read.—*London Mining Journal.*

MISCELLANEOUS.

POWER OF ELECTRIC LIGHT.—Late experiments at St. Petersburgh show that the power of the light may be increased by covering the carbon with a thin sheet of copper, and turning the cup towards the object to be illuminated. The most economical machine tried was that of Alteneck, which, with a galvanized carbon of 10 mm. diameter, gave a maximum of 20,275, and a mean of 14,039 candles. The light was sufficient to make objects visible, for military purposes, at a distance of 3080 yards. —*Nature.*

DEFENCES OF VICTORIA.—A Royal Commission appointed to consider the question of the defences of Victoria has recommended that the strength of the Naval Reserve formed by the colony should be increased with a proper complement of officers to 300 men, and that the men should be instructed in garrison drill and in the work of laying torpedoes, in addition to their duties on board ship. It is also suggested by the commissioners that a supply of material for stationary torpedoes should be procured without delay.

DAVYUM: A NEW METAL.—Serge Kern announces his discovery in June last, of a new platinoid metal which he calls *davyum*, in honor of Sir Humphrey Davy. It is hard, silvery in lustre, malleable at red heat, readily soluble in aqua-regia and very feebly in boiling sulphuric acid, yieding a yellow precipitate with caustic potash. Sulphureted hydrogen, passed through a dilute solution of the chloride, yields a brown precipitate which becomes black upon drying. Potassic sulphocyanide, with the same solution, is colored red, and if the solution of davyum in KCyS is concentrated, a red precipitate is obtained. Sp. gr. 9.385 at 25° C. Kern thinks that in Mendelejeff's proposed classification of the elements, davyum is the hypothetical element placed between molybdenum and ruthenium, in which case its equivalent should be 100. It would then rank as the second confirmation of Mendelejeff's predictions gallium having been the first. It is probably rare. The platiniferous sand does not contain more than 0.0045 of davyum.—*Comptes Rendus.*